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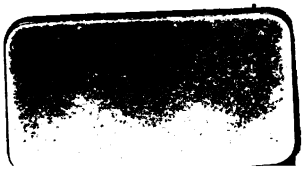

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TRANSACTIONS

OF THE

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PREFACE.

THE following Papers were read at a Congress of members of the medical profession practising homœopathy, held at Birmingham, under the Presidency of Dr. DRYSDALE of Liverpool, on the 28th September, 1870.

The attendance on this occasion comprised one-fifth of those medical men in Great Britain who openly acknowledge their confidence in the law of *Similars* as a guide to the selection of drug remedies.

Dr. DRYSDALE'S address, with which this volume opens, has already appeared in pamphlet form, while the remaining essays have been published in the *Monthly Homœopathic Review;* they are here collected, because, while presenting a clear view of the position occupied by homœopathy in the world of medicine, they afford an exposition of its principles as a therapeutic system, the correctness of which would, it is believed, be endorsed by the great majority of those who have studied and have had experience of homœopathic practice.

Dr. SHARP, in his essay on *The Action of Drugs,* describes his views of the operation of these agents upon the various tissues of the body.

Dr. MADDEN, in the next essay, *On the Study of the Materia Medica,* points out a very excellent plan for obtaining a clear insight into experiments made for the purpose of learning the *Action of Drugs.*

Dr. HAYLE, in discussing the meaning and relative value of symptoms, very happily described the light in

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which the homœopathist regards these indications of disease, and the mode in which he utilises them in prescribing for his patient.

Dr. BLACK, in his contribution to the proceedings of the meeting, illustrated, as it were, the practical application of the principles which had been enforced in the preceding essays.

It is hoped that these essays, while they cannot fail to assist the student of homœopathic medicine in his investigations, will also aid in correcting some of the erroneous notions which are but too widely spread regarding the principles upon which the practice of homœopathy is based.

EVAN FRASER, L.R.C.S. Ed.,
Hull.
J. GIBBS BLAKE, M.D. Lond.,
Birmingham.
} *Secretaries.*

December 1870.

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MODERN MEDICINE AND HOMŒOPATHY.

By Dr. Drysdale.

Gentlemen,—Fourteen years have now elapsed since a meeting of the general body of homœopathic practitioners such as the present has been held, and a generation has completed its term since the introduction of homœopathy into this country. It may therefore be deemed a fitting occasion to inquire how the medical profession of that first generation to whom it was presented have behaved towards it. That generation has had brought before it a law of the action of medicines which promised the discovery of their specific powers in disease, and by that means a beneficent revolution in the art of healing. At the same time evidence was given that, although the new method was comparatively in its infancy, the success already attained by it not only equalled but surpassed that of the existing methods. The public, for whom alone the medical profession exists, hear of these things, and, turning to the profession, naturally ask what is the true value of these statements, and what place this new law and practice deserves to take in the world of medicine as a whole. Hitherto, it is well known, all great discoveries have at first been pronounced upon and rejected by the medical profession without experiment or examination. That has been the invariable rule which the eager and often over-hasty adoption of small improvements rather confirms than invalidates. But surely things are changed now-a-days, since we hear nothing but boasts of the enlightenment of the men of the 19th century, and of their immense superiority to all their predecessors. Whoever is disposed to trust to this self-praise would be egregiously deceived, for it is a painful fact that the very same course has been followed in respect to this last, and, in many respects, the greatest of medical discoveries. I am not

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going into details of the history of the acceptation of homœopathy, as the subject would be far too extensive for our present purpose, and, besides, they are painfully familiar to you all; but I allude to the subject in order to show how the false step taken at the very beginning has an injurious influence even now on the general progress of medicine, as well as keeping us in the apparently sectarian position into which we have been driven by the unphilosophical conduct of the majority of the medical profession. The blame rests originally on the German medical contemporaries of Hahnemann, who was a man of truly scientific mind and undoubted learning, and who addressed himself exclusively to medical men presumedly capable of judging of his reasonings and testing his alleged facts. As soon as it became evident that his theories involved a very great change in medical practice, instead of continuing the discussion of them in the proper organs of scientific opinion, and testing the facts legitimately, a hasty judgment was pronounced against him, and the pages of the medical journals were closed to him. He was therefore forced to appeal to the small body of medical adherents who gathered around him, and the profession was at once divided into two parties, for which names and watchwords were speedily invented. I cannot but consider this unfortunate, and believe that if the medical profession had been actuated by a higher average degree of philosophy and the sense of justice, the name of homœopathy would never have been heard of by the general public, but the true place which the doctrine is ultimately to occupy in medicine would have been worked out within the medical body unknown to the public except through the change of practice induced. In England the same course has been followed, but with an aggravation which was not seen in Germany. In point of morality the Germans do not stand higher than our countrymen, and they have shown the same injustice in condemning unheard and excluding from the medical journals all homœopathic writing, and refusing up to this day to test the practice in the public hospitals. But, probably owing to their higher intellectual culture, they have not attempted to limit the freedom of thought by refusing degrees to homœopathic candidates, or to ostracise homœopathic practitioners by refusing consultations with them on neutral ground. These great faults, we are ashamed

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to see, have been committed in England, France, and America. As you are all aware, three medical corporations, who have the power of giving degrees, forgot their position so far as to refuse the license to practise to the candidates without an undertaking from them never to practice homœopathy, thus at one stroke degrading medicine into a sect. Soon afterwards the agitation began among the lower grades of the profession, which culminated in the famous resolutions of the British Medical Association at Brighton in 1851. This accomplished the degradation of the profession into a trades'-union, in so far as the profession is held to be represented by the majority of its members. By these resolutions, as you all know, the said majority bound itself to refuse all professional intercourse with—1st, those who practised homœopathy wholly; 2nd, those who practised it partially; and 3rd, those who should, in defiance of these rules, still consult with either of the two above classes. Likewise they were bound to withdraw consultation practice from the last, and oppose to the utmost their election to posts of honour and trust. This clause requires special attention from us at present. In support of these resolutions a number of special clubs, called ethical societies, were formed, and in many of the existing medical societies the homœopathic members were expelled and bye-laws framed against their admission in future. All this time, no definition of what was meant by homœopathy was given, but any one who expressed any opinion in favour of a philosophical investigation into its merits was immediately stamped as a heretic and persecuted accordingly. As if all this were not enough, a final step even more injurious to the fair fame of the medical profession was taken in the attempt to stifle and stamp out all mention of the subject in medical literature; in short, that most effectual (if possible to carry out) of all modes of persecution was put in force, viz., the conspiracy of silence. The editors and publishers of the medical periodicals were given to understand that the slightest sign of favour, or even the commonest fairness, towards the new doctrine, was to be the signal for stopping the sale of the publication. And this was no vain threat, for, among other instances, I have been credibly informed that within one week of the publication of Sir John Forbes's article in the *British and Foreign Review*, no fewer than 1,400 persons sent orders to stop

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their subscription. Pressure was also brought to bear on the booksellers, so that they were compelled to refuse to publish or even sell homœopathic books. These surprising instances of moral degradation and folly attracted the attention of the profession in Germany, and the following comment was made by the *Berliner Medicinische Central Zeitung* (an allopathic journal):—

"The agitation against homœopathy has given rise to excesses which are more than laughable—they are utterly contemptible. At the instigation of some fanatic medical men, a large publishing house (Highley and Son) have announced that henceforward they will neither publish nor sell any homœopathic works, and it is expected that other publishers will follow their example. This mode of attempting to stop the child's mouth is absolutely revolting, and all the more barbarous as occurring in a land where the right to give expression to opinion is sacred. That it must fail to be of the slightest use is so self-evident that we cannot comprehend the blindness with which Englishmen, who are generally held to be so calculating and practical, have acted in this matter."

These words were spoken in vain, as indeed were those of men in the highest intellectual circles in this country, including Sir Wm. Hamilton and Archbishop Whately, who publicly expressed their reprobation of the whole conduct of the majority, and denounced it as trades'-unionism. What have trades'-union cliques to do with reason or justice? Their very nature is to set such things at defiance, when in conflict with their petty interests and passions. I recall these matters unwillingly, but it is necessary, for all these restrictions are still in full force, and it is they which are now weighing heavily against the true progress of therapeutics in this country—now especially, when the practical use of the specifics, discovered solely through the homœopathic law, is beginning to be known and adopted by persons who still submit to the intellectually and morally degrading dictation of the allopathic sect. The profession was, happily, by no means unanimous in the course pursued, and there was a considerable minority who acknowledged the undignified and unphilosophical character of measures which would set bounds to the freedom of investigation in science. Among that minority it has been noticed that in each town were almost all those who had previously shown ability in the

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cultivation of the higher walks of medicine. The majority was mainly composed of those who follow medicine for a mere livelihood, and care nothing for science. Such forms, unfortunately, a low stratum which embraces a large part of the medical profession. I see the *Medical Mirror* pronounces that the bulk of the profession are not gentlemen, and I fear there is too much truth in this dictum, and that a considerable number merely obtain at the medical schools a thin varnish of gentlemanly and professional feeling, which is soon rubbed off. Many were then, and probably are still, obliged to get the chief part of their income from the sale of drugs, so it is easy to see how such persons would be disposed to resist a method apparently so prejudicial to their pecuniary interests. They naturally fell an easy prey to designing demagogues, eager to obtain a temporary notoriety, and increase their chance of consultation fees by an apparent zeal for orthodoxy. It has been remarked that the leaders of the persecution were almost exclusively men not distinguished in science, but merely bold, loud, unscrupulous, and especially vulgar persons; and it is a misfortune to any body of men to be represented by leaders who do not lead but merely seek personal notoriety by pandering to their petty prejudices and interests. One evil begets another, for the better class, from whom the consulting physicians had hitherto been chosen, are sorely tempted to enter in the race of bigotry and intolerance for fear of the trades'-union threat, above alluded to, being put in force against them. Some, though still, alas! only a few, have nobly vindicated the claims of honour and conscience; they see through the sophistical plea that there is no common ground for consultation in such different methods of treatment, and they refuse to persecute and insult the public by declining to give their aid in neutral ground, such as surgery and midwifery, on the plea that the family physician, tried and trusted probably for years, is a quack and an impostor. To such men all honour is due, for their devotion to abstract principles of right and justice, while they have not the enthusiasm of positive belief in a new principle which supports us. To the other portion of the liberal party who know the right and do it not, what are we to say? They are faithful, but not "faithful in the least." Possibly they persuade themselves that it is such a small matter that they are not called upon to sacrifice

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themselves in vindication of an abstract duty. Indeed, the only answers I have heard of from such men are two. One said, on application to meet a homœopathic practitioner in consultation, "Pray do not ask me. I know it is wrong to refuse; but I have already been so badgered and bullied on account of my opposition to the trades-union party that I am unwilling to fight the battle over again; so I beg you will not ask me." Another bluntly said, "I know it is wrong; but I don't choose to sacrifice my comfort and interests for homœopathy, which I don't believe in, and can't be troubled to study." It is not for us to condemn such men. We are all fallible, and possibly each of us may be acting so in things we as ignorantly deem of small import. The others, who do wrong with a high hand, we have a right to denounce in fitting terms. Such are some of the professional obstacles to the answer of the demand of the public; what is the true position of the homœopathic law in medicine? Against such pressure, need we wonder that the progress of the new doctrine is slow and difficult in this country? Should we not rather wonder that so many are still found in increasing numbers to fight the battle? I hope not, for I trust that the list of those in whom the claims of honour and conscience are paramount comprises still the great majority of the profession, though they have been betrayed into acting in the unworthy manner above described by trusting to the misrepresentations of homœopathy palmed off upon them by unscrupulous partisans, and neglecting to examine the question for themselves. We have all here passed through the same stage more or less, but have been in different ways awakened to the sense of personal responsibility which lies equally upon all medical men. In no instance has the true nature of homœopathy, and the position of the homœopaths as a scientific school, and not a sect, been displayed in such a striking and almost dramatic manner as in the conversion of Dr. Horner. That gentleman, you may remember, was physician to the Hull Infirmary, and so highly respected that he was chosen president of the British Medical Association in the year of the celebrated anti-homœopathic resolutions. In the chair he doubtless had to put them to the meeting, and also to sign them. But when the heat and noise, the stamping and confusion of the meeting were over, and he retired to solitude, then the still small voice of conscience

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began to be heard in this wise—Do I really know what is implied by the undefined word homœopathy; have I taken any steps to ascertain whether it is truly "opposed to science and common sense as well as at variance with experience?" If not, have I any right to say its professors are fools, cheats, and impostors, and ought to be cut off from all intercourse with the profession? When asked by the public, dare I testify to this effect without haply bearing false witness against my neighbour? This is the cardinal test to which we have all been put. No one now living discovered homœopathy for himself, nor even learned it from books alone; but we have all come in contact with some one equal, if not superior, to ourselves in ability, knowledge, and honour, who has studied the subject practically, and found a measure of practical truth therein. Could we denounce him as a quack, or meanly keep silence on the subject, or must we not examine whether things were so? As to us, so to Dr. Horner: conscience would allow no rest till he had diligently read the best accessible works on the subject. He was then astonished to find how ignorant he had been of what homœopathy really was. He next proceeded to test, by practice, the truth of the statements he had read. "In my practical inquiry," he writes, "into the powers of homœopathic remedies, I resorted to every species of proof which I could devise, and of which the subject seemed capable." The result was that in 1857 he published a pamphlet entitled "*Reasons for Adopting the Rational System of Medicine,*" the rational system being what is called homœopathy, and which he continued to practise till the time of his lamented death. The immediate consequence, however, was that, like Dr. Henderson before and Dr. Reith since, he was turned out of his post in the infirmary. The experience (except, I hope, in the last matter) of every one who listens to the voice of conscience and examines the subject before condemning it, will be exactly the same as Dr. Horner's. He will find that the homœopathic body are no sectarians who blindly follow a mere dogma irrespective of the claims of all other results of scientific experiment. It is no system of covering the subjective and often trivial symptoms of disease with merely subjective and often false and ridiculous symptoms, attributed to the physiological action of medicines which are then given by sniffing a globule of the decillionth dilution, as the sole

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practical outcome of the physician's art. It is only possible to recognise this caricature as applied to the abuse of homœopathy, because it is the only method which takes cognizance of the relation of the minute shades of the action of medicines to those of disease. The homœopathic body are simply medical men who seek the improvement of the art of healing by studying the action of medicines on the healthy body, and applying the knowledge so gained to the treatment of disease by the homœopathic law of specific relationship. This law applies exclusively to the vital actions of medicine when they correspond directly to the purely vital actions deranged in disease. There is, therefore, a certain field where the principle is not applicable, and there we must use and do use exactly the same remedies as other medical men. Among these are all dietetic and chemical means, evacuants, stimulants, &c., which may be needed for removing exciting causes and restoring nutrition. The boundary line between specific and other medication has not been and is not yet fixed; accordingly we are prepared to abandon attempts to treat specifically ailments which a better knowledge of their nature shows not to be within the scope of specific treatment. Of this we have several conspicuous examples. When the true nature of parasites was discovered, we at once abandoned the treatment of them exclusively as diseases, and began to give vermicide remedies in full dose. Likewise, we were among the very first to discern and adopt the use of carbolic acid and antiseptics. In the large and fatal class of zymotic diseases, although the homœopathic law applies conspicuously in their prevention, we have hitherto failed to find how the contagious principles can be adapted to the cure of the fully developed disease; and if hereafter any agents should be discovered capable of curing it by destroying the contagious matter within the body, we are prepared to adopt it with gladness, though it be not homœopathic. In short, we simply use the homœopathic principle where applicable—neither more nor less—according to the best of our ability. And in any case we simply use it because it is better than any other, and are ready to give it up for a better. We are exactly, I hope, in the position of men of true science—seeking to apply the homœopathic law wherever experience shows it to be applicable; not bound together by a sectarian dogma to be followed

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blindly, though we can accept no contradiction not backed by experiment. Although there are differences of opinion within our body, still I think it will be admitted by this Congress, as it is likewise by that ornament to his profession Dr. Caroll Dunham, of New York, that no more stringent tests should be given of what constitutes a homœopathist than that it is simply every one who accepts the *similia similibus* as the law of specifics. In all other respects every individual should be free to judge for himself. All acknowledge that ultimately the name of homœopathist must cease, and many think it ought to be discarded now. On this point I would recall the opinion of the distinguished pathologist Tessier, whose adoption of our method gave such an impetus to its progress in France:—

"Yes, we refuse, and always shall refuse, the appellation of homœopathists, because we are as adverse to the sectarian spirit as we are devoted to truth. We admit to be true the therapeutic system, invented and developed by the genius and immense labour of Hahnemann; but just as we adhere to the psychology and physiology of St. Thomas, without calling ourselves Thomists; just as we profess the doctrine of the essentiality of diseases, without calling ourselves essentialists; just as we cultivate, and have always cultivated, pathological anatomy, without calling ourselves anatomical pathologists; just as we employ auscultation and percussion, without calling ourselves auscultators and percussors; just as we devote ourselves to clinical observation, without calling ourselves observing physicians; and just as we study general medicine, without calling ourselves theoretic doctors, so we neither desire nor admit the appellation homœopathic applied either to ourselves or our labours. We are physicians; and our work is to perfect the art of medicine. We admit homœopathy, because it is a great truth in therapeutics; and we reject it in the character of a medical doctrine."

I think we must all agree abstractedly in the propriety of these sentiments, but I fear we are bound practically to retain or rather submit to the name from motives of honour, lest we should seem even to countenance the mean subservience of certain persons who secretly adopt the fruits of the labours of our predecessors, and deny the source for fear of the threats of the trade unionists. The name also must remain for a distinctive literature as long as the exclusion of homœopathic papers and

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books is maintained by the allopathic journals and booksellers. I have offered frequently, and hereby renew the offer, to use any influence I may possess to stop the publication of the *British Journal of Homœopathy* as soon as freedom of publication is given to homœopathic writers in the ordinary medical journals. Of the injury done to the progress of medicine as a whole by the sectarian exclusiveness of the allopathic party I shall have to speak presently, but I may here notice that it has been equally, if not more, injurious to us to be deprived of the purifying fire of an enlarged and enlightened criticism. That has been our great want, for in a small body bound together by the ties of common suffering through persecution, there is naturally too great tenderness for the individual, and much work has passed muster or even received praise which would have been sternly rejected under a system of juster criticism. I feel convinced that if Hahnemann had not been driven from writing in *Hufeland's Journal*, his *Organon* would not have assumed its present dogmatic shape, nor would he have pushed some of his more hypothetical views to those extreme lengths which have been a perpetual source of embarrassment to nearly all of us ever since. Thus, again, we see how evil begets evil, and a great part of the apparent narrowness and sectarianism of homœopathy is chargeable to the really narrow and sectarian attitude of allopathy as it writes itself down. In view of the foregoing considerations I think we may fairly claim as a body to be the only men who are in the position, according to the strict rules of philosophy, to answer the question, "What is the true place to be ultimately occupied in medicine by the homœopathic principle?" We cannot do it yet, as it requires time and numbers, and in fact the homœopathic principle can only be fully applied in proportion to the advance of general medical knowledge which culminates in semeiotics. Till we know the *ratio symptomatum* of every group of symptoms, how can we be certain that any two groups are really similar to each other? Be it remembered that it is to the pathological simile alone that the homœopathic law applies, and not to any mere superficial resemblance of a few symptoms common to many morbid states. We have, happily, enough of examples of true correspondence to prove the truth of the law of cure, and to attain to a great degree

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of average success in the treatment of disease. But we are as yet—medicine, as a whole, is very far as yet from that perfection of the semeiotics, both of disease and of the physiological action of medicines, which shall enable us with unerring certainty to show the correspondence of the two in any given (small) number of cases. Therapeutic experiment, in the eyes of all competent judges, is surrounded with so many elements of uncertainty, that no one with any pretension to true science will look for decisive results, especially negative ones, from a few trials. According to the well-known dictum of Andral—"With thirty or forty observations one may determine the diagnosis and pathological anatomy of a disease, but it needs years of research to arrive at a satisfactory result in therapeutics." To whom, then, is it given to try this great question in the true temper of philosophy, viz., that which seeks truth only, and for its own sake alone? Is it to us who are no sectarians, and ready to accept any answer which nature returns to our question, and who have bought our freedom at the price of life-long persecution? or is it to those who have bound themselves by a solemn declaration that they will not look in one direction, and there is one answer they will not receive from nature? or to those, if not themselves so bound, who still fear the animosity of the sectarians and bigots? In the present state of medical literature, who would look for a statement favourable to homœopathy in any allopathic journal, but more especially in the weekly medical newspapers, viz., the *Lancet, Medical Times,* and *British Medical Journal?* It would be the signal for loss of sale and the immediate dismissal of the editor; and how can we expect an unbiassed judgment from an anonymous hireling, trembling for his piece of editorial bread? Likewise, no expensive medical work could at present be published if it contained anything favourable to homœopathy: accordingly the subject is entirely ignored, or, if alluded to, must be mentioned with reprobation. These books may abound in plagiarisms from our school, and its influence may be seen in every page almost, yet the subject must not be named. The private and hospital practice of a physician may display in almost every prescription the fruits of knowledge gathered by our school, and yet he dare not honourably acknowledge their source under the penalty of loss of his appointments, expulsion from some medical societies,

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and general professional ostracism. Are the writers in such books, and are such practitioners, in a position to weigh fairly a difficult question like this in the true philosophical temper of mind? In therapeutic experiment we may fail twenty times to get a result perfectly crucial and free from doubt, and yet succeed on the twenty-first trial. Would not a person hampered as above be sorely tempted to accept the twenty negative results as sufficient evidence to let him retain his ease and comfort, honour and emoluments with a clear conscience? It may be that in all those books and periodicals, and in medical practice, the authors and practitioners have taken such steps as to justify themselves in ignoring homœopathy with a clear conscience, but assuredly the *onus* lies on them to show that it is so. For this they have to thank the trades'-union resolutions of the British Medical Association.

These remarks on the ethics of our position have extended beyond the limits I expected to allot to them; and though the subject is far from exhausted, little time is now left for the consideration of the actual state of modern medicine in regard to homœopathy. I had intended to discuss the *bonâ fide* difficulties in the way of recognising the homœopathic law in the action of the specifics used empirically in ordinary practice, and also how far the empirical use of homœopathic remedies borrowed from us without acknowledgment, and given singly on clinical indications alone, which is becoming practised in some London hospitals, can lead to certainty in practice. This is a very interesting question, which was handled by Hahnemann in an essay which might be read with profit at this very day by any hospital physician who has begun to think on pure experiment with medicines, and hopes to find out specifics *ex usu in morbis* alone. I perceive, however, that Dr. Madden has a paper which will, no doubt, embrace this subject, and set the question in its right light both ethically and scientifically with the clearness and power which we recognise in all his papers. The other question is, I fear, too large for an occasion of this kind, so I will pass to some concluding remarks on the state of modern medicine.

Nothing can be more gratifying to us than to see the direction in which medicine is now proceeding, as realising all our previsions and adopting one by one the principles and practice for which we have been contending.

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One by one the negative advantages of the homœopathic method have been recognised and adopted, solely owing to the influence of our example. It is true, it is alleged at the same time that we merely demonstrated them incidentally and involuntarily owing to the nihilism of our practice. But we know to the contrary, and were always able, nearly though not quite, to discriminate what was negative, while the false estimate of our practice has betrayed the sceptical school of our partial imitators into a simply expectant practice, in which mere removal of the exciting cause and the attainment of physiological rest, with the use of anodynes, is all that is attempted; so that, in fact, the physician is little better than a cook or a nurse; or else it has betrayed them into a dangerous revulsion, into the stimulant practice of the Brunonians. When we look at any *résumé* of the recent progress of therapeutics we find the summing up to be excessively meagre—positively nothing but the use of antiseptics, which we adopt equally, and the discovery of new narcotics and anodynes, as if the mere palliation of suffering was all that medicine pretended to. To be sure, there is the fashionable drug the *bromide of potassium*, which, however, is chiefly praised for its merely palliative action on the nervous centres. Beyond these no systematic gain to therapeutics can be named. But when we look into details we discover a great change going on, viz., a most extensive use of specifics, *i.e.*, medicines whose operation can be traced to no obvious physiological action taking place during the process of cure. These are generally now given unmixed, ostensibly empirically and from clinical indications alone, and in comparatively small doses; so that in some clinics we might at first sight almost fancy we had got into a homœopathic dispensary by mistake. We hear nothing, however, of homœopathy, and no reason is given for the choice of the medicine, only somehow or other it was found to be good, and is repeated empirically. What can this mean? We know very well how it was found to be good. Did the prescriber really not know, or did the above spoken-of threats induce him to keep silence? But we see also another phenomenon. For the last sixty years the homœopathic body have proclaimed incessantly the necessity of proving medicines on the healthy body. Lately, we have had this recommended, and grants of money proposed for its accomplishment, but

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no word of homœopathy! Moreover, it has been partially carried out, and one bulky book has been published on some medicines with no word of homœopathy except a passing sneer. It is still more remarkable that when the author comes to grapple with the results of the provings, much of his reasonings and his conclusions are found actually identical with our own; yet there is no word of homœopathy. I allude to Dr. Harley's *Old Vegetable Narcotics*. In this book the remarks on *belladonna* are especially interesting to us, for we find that the further the author advances in his study of its action on the healthy body, the more closely do his views of it approximate to those of our school. He recognises the similarity between the primary action of *belladonna* on the capillary vessels and sympathetic nerve, and the states of fever and inflammation, and, in consequence, anticipates "that its sphere of usefulness will be acknowledged before long to be co-extensive with that of acute disease itself." He then proceeds to explain and illustrate its curative action in these words:—

"The action of *belladonna* in febrile diseases is frequently attended with results which are not only unexpected, but exactly the opposite of what is observed in health. Thus it may happen, if we give a full dose of *atropia* to a patient with a pulse of 120 and higher, a dry and hard tongue, and pupils measuring $\frac{1}{6}''$, that after 10, 20, or 30 minutes, when the action of the *belladonna* is fully developed, the pulse will be decreased, the tongue be moist, and the pupils contracted. Two similar effects, the one arising from a local irritation and the other from the presence of *belladonna*, like spreading circles on a smooth sheet of water, interfere with and neutralise each other. The coincidence of the two actions and a corresponding augmentation of the effects may be possible, but this I have never witnessed. It appears, therefore, that the stimulant action of *belladonna* is converted in great measure in febrile diseases into a tonic and sedative influence."

This is simply the language of homœopathy, and is all that we have been contending for in theory and practice for years. The very illustration of the mutual extinction of waves of similar periods is one that has been used several times independently by different writers—by myself among the number in the year 1846. But, besides this, Harley dissociates the sympathetic fever from local

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inflammation, and maintains that it can be cured independently of the local inflammation, and on its removal the latter is quickly relieved. This, again, is nothing but the theory and daily practice of the homœopathic school. We assume to have no specifics for concrete diseases, but remedies for certain elementary morbid states, which are, as it were, dissected out by different medicines in the different phases of a disease. As, for example, *aconite* or *belladonna* is given at the early stage of inflammatory and feverish diseases, to be followed by the local specifics at later stages. It is possible Dr. Harley does not know all this. We gladly give him the benefit of the doubt, but we must remind him that the *onus* of disproving the charge of plagiarism always lies with the assumed rediscoverer. We hope and trust that after proper study of the question he will follow the example of Dr. Horner and Dr. Reith.

Liverpool,
September 1870.

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THE ACTION OF DRUGS.

By Dr. SHARP, F.R.S.

"A man is not permitted without censure to follow his own thoughts in the search of truth, when they lead him ever so little out of the common road." LOCKE.

PROFESSOR TYNDALL commenced a letter to the *Times*, a few months ago, with this remark: "*The theory of disease* was never discussed with more earnestness, or with greater precision, than at the present time." This is true of the old school of medicine. I hope it may be said with equal truth that, in the new school, the *theory of cure* is discusssed with the same earnestness and with the same precision. The thing to be desired is, that each school would lay aside all exaggerated condemnation of the other, and that they should work in harmony together. Each school might render valuable help to the other.

There are many ways in which a patient may be assisted in his recovery from illness; but when we speak of the *cure of disease*, we commonly mean a cure effected, more or less, by the administration of drugs or medicines. A true theory of cure, therefore, cannot be propounded until a clear account has been given of the action of drugs.

What are *drugs?*

If we look at all material things which surround us, with reference to the life and health of man's body, they arrange themselves in three classes. One class constitutes *food*, and contains all substances which, when digested, form blood, and so nourish the body, and sustain its vital power. Another class consists of *stimulants*, which do not make blood, nor nourish, nor sustain, but which help in the use and concentration of the living power previously possessed. Another class is composed of a number of remarkable substances which are not food, nor diffusible

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stimulants, but which act injuriously in health, and curatively in disease—these are *drugs*. When taken in health they are often called *poisons;* when taken in illness they are commonly called *medicines*.

A few words about food and drugs. Drugs may be said to be the opposite of food. They cannot nourish nor support life. When taken in health they are injurious to it, and many of them have power to destroy life. But it is known, and apparently it has been known in all ages, that when in sickness they are opportunely taken, they have power to relieve pain, and often even to remove disease, and to restore health.

The difference, therefore, between food and drugs is very great. Food is required to make blood, to provide materials for the growth of the body, and for the repair of its daily waste. It must be taken daily, and in considerable quantities. Drugs are not needed in health, but in sickness; and then only in such small quantities as shall be found sufficient to alter morbid action in the direction of health.

That drugs, which are poisons to the healthy, are remedies to the sick, is a statement suggestive of many enquiries. An active imagination may speculate on the questions *why*, and *how*, to an indefinite extent, but probably with little profit. Medicine is a science of fact. It has been made the sport of speculation. To place it in its true position should be the aim of its students.

Facts are living verities, and he will be the best physician who has the largest acquaintance with facts in medicine, and the happiest tact in remembering them, as they are needed to guide him in the treatment of his patients.

And what is the *action* of drugs?

Sir Thomas Watson, the present head of the profession in England, lately said, in an address to the Clinical Society of London, "We want to learn distinctly and clearly, *what is the action of drugs*."

If we enquire what facts are known which help to furnish an answer to Sir Thomas Watson's question, these subjects will require investigation:—

I. In what way is the action of drugs to be discovered?

II. What is the action of drugs?

III. How is the action of each drug to be distinguished from that of all others?

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As it would be impossible to consider these three questions in twenty minutes, the time at present at our disposal, we will confine our attention to the last—*How is the action of each drug to be distinguished from that of all others?* *

First, in health.

The primary fact, forming the basis of the investigation, is this—all drugs have a partiality in their action; a preference to some parts of the body above others.

The next fact—this preference varies with each drug; and sometimes even with different quantities of the same drug. In other words, an affinity exists between different parts of the body and different drugs, or doses of drugs, which affinity produces actions which are more or less characteristic in each case.

This affinity, together with its effects, lies at the bottom of the use of drugs as medicines; and of every theory which has been advanced in explanation of that use. All schools of medicine avail themselves of it alike. It gives rise to some of the most surprising and interesting phenomena in nature.

How wonderful that a few drops of the juice of one plant should, when taken by a healthy person, produce a morbid affection of the brain, and a few drops of another, a disordered condition of the lungs; that a third plant should act upon the stomach; that a fourth should attack the heart; a fifth, the liver; a sixth, the muscles; a seventh, the joints; and so on, through every part of the body.

In this manner *a first line of separation* between one drug and another is drawn distinctly.

But several drugs are found which act primarily and powerfully upon the same part or organ of the body. In this case a distinguishing feature lies in their *manner* of acting. Generally this is very different, sometimes it is directly opposite.

This is a *second line of separation.*

Again, drugs which select the same parts for their primary action, act also upon other parts or organs in a secondary manner. And this secondary action turns out, for the different drugs, to be upon different organs. Hence we have, for those drugs whose primary action is

* Perhaps I may remark that the two former sections of this essay are partly written.

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on the same part, but whose secondary action is on different parts, a *third line of separation.* This forms an easy method of distinguishing them from each other.

A careful observation of these three differences—the organs acted upon primarily or most powerfully—the kind of action—and the organs acted upon secondarily or less powerfully, will be found to be sufficient to distinguish one drug from another.

Time will not permit me to give examples, but they will be familiar to you.

Let us turn for a moment to look at the diseases for which we use these drugs, and at the various causes which produce them, and we shall see three facts.

The common causes of disease act with a partiality or power of selection. The primary or most powerful action being on some particular parts or organs of the body.

This action is special in its kind or direction.

Other organs are acted upon in a secondary or less powerful manner.

Here are two parallel sets of phenomena. Drugs, in fact, act upon the healthy body very much as other causes of disease are seen to act; and they differ from each other as other causes differ. They are to be numbered among the causes of disorder or disease in healthy persons.*

Such, I venture to think, is the answer to the question, How is the action of each drug to be distinguished from that of all others when taken in *health?*

We will next consider the question with reference to medicines, or drugs taken in *sickness,* and we shall observe a similar series of facts.

Drugs taken in sickness show a partiality in their action; their primary effects appearing in some parts of the body, and not in others. They differ from each other in *the parts selected* for this primary or most powerful action. In this manner they are distinguished from each other.

They differ also in their *kind of action.* It may be remarked generally, that large doses act as medicines in the *same* direction as that in which they act as poisons, and small doses in an *opposite* direction.

They differ from each other in their *secondary* or less powerful action.

* It is an obvious inference from this fact that, except for scientific experiment, healthy persons should never take medicines.

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Here is a series of phenomena parallel with that produced by drugs taken in health. They are parallel but not identical phenomena.

The link which connects the proving of, or experimenting with drugs in health, with the use of them as remedies in disease, is this fact, that

Drugs select *the same parts* in disease that they do in health, both for their primary and for their secondary or subordinate actions.

This, I hope, is a satisfactory answer to the question in its second aspect. How is the action of each drug to be distinguished from that of all others, when taken in *sickness?*

Suffer me now to offer three remarks:—

The fact of selection itself opens the door to prescribing drugs as medicines with two opposite intentions. They may be given to act upon the healthy, or comparatively healthy parts, and so as to avoid the diseased parts. Or, they may be given to act upon the diseased organs, and so as to leave the healthy ones untouched.

The fact that the direction of the action of medicines is sometimes similar, and sometimes opposite to that of poisons, according to the dose given, renders it possible to use them antipathically or homœopathically.

While the fact that the secondary, or less powerful action of a drug, is on the same parts, whether given in health or in disease, greatly facilitates the choice of the remedies for the individual patient.

This is the outline of the subject; but before it is exhausted, other parts of it will require to be settled.

One of these is the peculiarity in the character of each drug, which lies in the *quantity* or dose in which its various actions are produced. Unhappily, this part of the subject is yet involved in much perplexity.

Another point is an enquiry into the *limits* within which the *curative* action of each drug is confined. Certainly the power of drugs over disease is limited. This fact seems to have had little attention bestowed upon it, having been forgotten by physicians as well as by patients and their friends.

Again, a bye-path in this search after the knowledge of drugs would lead us to the observation of some curious

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facts relative to the mode of *applying* drugs (as well as to the preparing of them for application), in order to obtain their action. Some drugs act when applied to the skin, as *arnica, rhus, ranunculus, cantharides;* others when applied to the mucous membranes, but not when put upon the skin, as *aconite, briony, pulsatilla;* others require to be introduced by a wound into the circulation, as vaccine, and some other animal poisons. *Belladonna,* if rubbed round the orbit, will dilate the pupil; *calabar bean,* similarly applied, will contract it.*

It may be asserted that the primary action of drugs is upon the 'vital principle."

HAHNEMANN was a vitalist, following in this the school of VAN HELMONT and BARTHEZ. He announces his adoption of these views at the commencemont of his *Organon of Medicine:—*

"In the healthy condition of man the spiritual vital force, the dynamis that animates the material body rules with unbounded sway, and retains all the parts of the organism in admirable, harmonious, vital operation, with respect to both sensations and functions, so that our indwelling reason-gifted mind can employ this living, healthy machine for the higher purposes of our existence." § ix.

"When a person falls ill, it is only this spiritual, self-acting vital force, everywhere present in his organism, that is primarily deranged by the dynamic influence upon it of a morbific agent inimical to life; it is only the vital force, deranged to such an abnormal state, that can furnish the organism with its disagreeable sensations, and incline it to the irregalar functions which we call disease." § xi.

Medicines cure by also possessing a spiritual power:—

"This spiritual power, capable of altering man's health, (and hence of curing diseases), which lies hid in the inner nature of medicines, is not, of itself, discoverable by us in any way by a mere effort of reason; it is only by experience of the phenomena it displays." § xx.

This is imaginary. It is acknowledged to be so by BARTHEZ himself. These are his words:—

"We can only give negative assertions, doubts, and conjectures on the nature of the vital principle in man."

* I have applied a considerable number of powerful drugs in the same manner, without being able to add to these two another which produced either dilatation or contraction of the pupil.

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Such speculation appears to me unpractical and useless; if I may venture to say so, worse than useless; because it is covering ignorance with the clothing of knowledge.

Let me not be misunderstood. I do not doubt that, in addition to an immaterial and immortal soul, man has an animal life in common with the brute creation. But I doubt the utility of speculations on this animal life in a medical sense; and especially when a similar kind of spirit is ascribed to drugs. We know too little of this animal life for a profitable discussion of it at the bedside of the sick.

It may be asserted that the primary action of drugs is molecular, and that we ought to start from molecules or atoms, and their movements.

I believe that molecules, and molecular movements, are real existences, and that infinitesimal doses, and their action, belong to this category; though we cannot follow them with the eye, even aided by the microscope. This subject is entertained in the Essay on "the Small Dose," written eighteen years ago. It has been well handled, just now, by Professor TYNDALL, in his lecture before the British Association, at Liverpool, on the 16th of this month. "There is," he says, "a world of matter and of motion to which the microscope has no passport, and in which it can offer no aid." "The first marshalling of the atoms, on which all subsequent action depends, baffles a keener power than that of the microscope."

Possibly the spectroscope may be an instrument better fitted to tell us something about these atoms and their movements. But, up to the present time, we know little, or nothing about them, much less can we distinguish one from another. We cannot, therefore, penetrate thus far into the action of drugs.

When we descend into more visible and tangible regions, it may be asserted that the primary action of drugs is on the blood and its heterogeneous particles, upon which each drug produces its particular change. I believe it is; but here also, except to a very limited extent, both our microscopes and our chemistry fail us. Notwithstanding the wonderful minuteness and delicacy to which both these methods of research have attained, they are not yet able to compete with the subtlety of these changes. There is sufficient encouragement both to the microscopist and to the

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chemist to carry on the enquiry; but it cannot yet be made the starting point to the therapeutist in his explanation of the action of drugs.

Once more; it may be asserted that all drugs act primarily upon the nervous system, each drug selecting particular nerves; *e. g.*, ipecacuanha the vagus; platinum the trigeminus; colocynth the sciatic. This assertion may be true. I heartily wish our present provings of drugs were complete euough to put us in a position to investigate it thoroughly, and to master its details. This would be a vast stride in advance in the pathogenesis and therapeutic use of drugs. And further, we are indebted to Sir Charles Bell for teaching us that all nerves have special powers, some being nerves of sensation, others nerves of motion; and that when an organ performs several functions, it receives nerves from several sources. To be perfect in our method of prescribing drugs, on the theory of nerve action, we should know how to act upon each of these several kinds of nerves. But our present knowledge in this direction is far too limited to be practically useful.

Finally; we are shut up to the conclusion that, if we would advance a step beyond the mere mechanical enumeration of symptoms, and prescribing for them —a method which satisfies some, but which is very far from satisfying all—we must look to the *seat* of these symptoms, the parts of the body whence they proceed. We must look, in other words, to the pathology of our cases, and try to discover the organs morbidly affected, and the kind of affection. And seeing that, with our present means, we cannot go beyond this in the examination of drugs, we must be content to concentrate our attention upon the organs themselves, as supplied with nerves and blood, and to learn the action upon these organs of the several drugs we use.

And, assuredly, to determine as correctly as is possible, the *locality* and the *nature* of the action of every drug; and also the *limits* of its *curative* action, are problems of the highest importance.

With these limitations, how is the action of each drug to be distinguished from that of all others?

The answer, as we have seen, involves the observation of phenomena of various kinds. We have to learn

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1. The *organs*, (solid and liquid), of the body upon which each drug acts *primarily*.
2. The *parts* of the organs in which this action occurs.
3. The *kind* of action, or the results produced.
4. The *organs* which are affected *secondarily*.
5. The *quantities* or doses required.
6. The *limits* of the therapeutic action.

As an example of the practical carrying out of these views, so far as regards the four first of these series of phenomena, I beg to invite your attention to the diagram before you. It is necssarily brief and imperfect, and probably, in some respects, erroneous; but I hope it will convey an idea of what I think is required to be done for every organ, and for every drug.

LOCAL ACTION OF DRUGS.

BRAIN.	ITS PARTS.	KIND OF ACTION.	SECONDARY ACTION.
Belladonna.	Arteries.	Inflammation.	Throat, eyes, &c.
Opium.	Veins.	Congestion.	Heart, nerves.
Hellebore.	Absorbents.	Effusion.	Serous membranes.
Hyoscyamus.	Cerebral substance.	Visions.	Eyes, heart.
Cannabis.	Do.	Convulsions.	Urinary organs.
Narcissus.	Do.	Pain, insensibility.	Stomach, uterus.
MIND.*			
Anacardium.	Memory.	Failure of memory.	Nerves of the five senses.
Aurum.	Imagination.	Dread of evil.	Reproductive organs.
Oleander.	Thought.	Inability to think.	Motor nerves.
Cotyledon.	Emotions.	Suppressed emotions.	Heart, lungs.
Ignatia.	Do.	Sorrow.	Rectum, spleen.
Mercurialis.	Do.	Excitement.	Mucous membranes.
HEART.			
Aconite.	Arteries, muscle.	Excitement, depression.	Arteries. larynx.
Digitalis.	Left side.	Depression, excitement.	Kidneys.
Bovista.	Walls.	Hypertrophy.	Skin, uterus.
Spigelia.	Valves.	Valvular disease.	Eyes, ears.
Lachesis.	?	Chronic disease.	Throat, bowels.
Bromine.	?	Do.	Larynx, eyes.
BLOOD.			
Ferrum.	Red globules.	Red globules increased.	Heart, arteries.
Rhus.	?	Typhus.	Joints, muscles.
Lycopodium.	?	Pus.	Liver, intestines.
Titanium.	?	Albumen.	Eyes.
Uranium.	?	Sugar.	Kidneys.
Petroleum.	?	Mucus.	Do., intestines

* So far as this is influenced by the brain.

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Before concluding, suffer me to offer a few remarks on the different forms of antagonism, with respect to the use of medicines, which exist between the two schools into which the medical profession is now divided.

Both schools take advantage of the local action of drugs. The first antagonism, briefly stated, is this :—

The old school says :

Avoid the diseased organs, and act upon the healthy parts. This is called *revulsion* and *counter-irritation.*

I copy from my notes the conclusion of one of Dr. ARMSTRONG'S lectures—the most eloquent and popular teacher of the practice of physic in London forty-five years ago—

"Practical application.

"1. Make out the internal conditions, by investigating minutely the combination of symptoms.

"2. Restore the diseased parts, by acting upon the sound ones."

This method is carried out in the Materia Medica of Dr. PEREIRA. To give one example :—

"*Gamboge.*—USES.—It is a remedy well adapted for acting as a stimulus to the abdominal and pelvic viscera . . . to give them preternatural activity, and thereby to relieve *some distant organ,* on the principle of counter-irritation."

The new school says :

Avoid the healthy organs—let well alone—do not make artificial diseases—act upon the diseased organs ;

e.g., give *opium* in apoplexy, *cantharides* in inflammation of the bladder.

The second antagonism lies in this :—

The old school now not unfrequently says :

You may act upon the diseased organ, but antipathically ;

e.g., Dr. GRAVES recommends *belladonna* to be given to counteract that condition of the brain which is accompanied with *contraction* of the pupil.

The new school says :

Act upon the diseased part homœopathically ;

e.g., give *belladonna* for a *dilated* pupil, *opium* for a *contracted* one.

The third opposition. It is now said, in effect :

Prescribe homœopathically, but give large doses, and discover a pathological explanation.

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This looks like an unwilling testimony to those who say:—

Prescribe homœopathically, and give small doses; acknowledge the obligation, and bear the odium.

I will detain you with but one further remark; it will bring us back to the sentence with which we commenced.

Dr. GULL, in a discourse "*On clinical observation in relation to medicine in modern times,*" delivered at a meeting of the British Medical Association, at Oxford, in 1868, and published last year, in the "Medicine of Modern Times," gives an elaborate review of pathology and diagnosis—the *theory of disease.* He then sums up what he has to say on therapeutics—the *theory of cure*—in words of serious import—I might almost say, of melancholy confession—as showing how little, in what is vainly called "orthodox medicine," the conclusion of the whole matter amounts to.

"The surgeon," says Dr. GULL, "is contented to place a wounded part under the conditions of physical and physiological rest, and, after attention to hygienic conditions, to abide the result. This, no doubt, expresses the largest part of *our* treatment of common acute disease. We now *know that we cannot directly control* the morbid processes in pneumonia, pleurisy, or pericarditis; we *know further,* that the means formerly considered essential to the cure of these diseases, tested by better clinical observations, *were either useless or pernicious.*"

Now the new school, with a consciousness of the solemn responsibility of the statement, and denying vain-glorious assumption, deems it its duty to say—we do know that we can, when we have a fair opportunity, often "directly control the morbid processes in pneumonia, pleurisy, and pericarditis," and many other forms of acute, as well as of chronic disease.

Thus encouraged, let us persevere in the daily study of the local action of drugs, not forgetting any longer the limits of their curative action. How much over-drugging—how much useless and even hurtful medication—how much unavailing effort and bitter disappointment will such knowledge put an end to!

Rugby, September, 1870.

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HOW TO STUDY THE HOMŒOPATHIC MATERIA MEDICA.

By Henry R. Madden, M.D.

Mr. President and Gentlemen,—I feel that the subject I have chosen for my paper is one whose extreme difficulty is only equalled by its importance. Ask any earnest homœopathist wherein lies the greatest obstacle to his progress, and he will assuredly reply, "The extent and complexity of the Materia Medica." It is here that he finds himself opposed by an almost insurmountable barrier; and the very riches with which he is surrounded appear to condemn him to perpetual penury. No question has been more frequently asked than "How shall we discover a key wherewith to unlock the treasure-house of our accumulated provings?" No effort has been more often or perseveringly made than to provide a clue to the labarynthine intricacies of our symptomatology, and yet, after fifty years of labour, the same difficulty is felt to exist.

I need hardly remind you that the success or failure of any attempt to overcome an obstacle, or to unravel an entanglement, depends more upon the method adopted than on the amount of perseverance bestowed upon the effort; and in the case in question, I am convinced that the want of success has been in a very great degree owing to the manner in which the subject has been handled.

During the last twenty-five years two very interesting papers have been published on the study of the Materia Medica, viz., one by Dr. Hering of Philadelphia, in the second vol. of the *British Journal of Homœopathy*, and another by Dr. V. Meyer, in the second vol. of the *North American Journal*. Dr. Hering recommends the thorough and complete study of one medicine as the starting-point, and then advises that the student should take some analogous remedy, and work out carefully the resemblances and differences which exist between these. He is then directed to treat a third drug in the same way, and in this manner gradually accumulate a store of knowledge. Dr. Hering assures us that the main difficulty consists in

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the thorough study of the first medicine, and confidently asserts that every subsequent remedy will be understood with less and less difficulty, so that at last a single reading of a proving will enable the student to grasp at once the essential characteristics of the new drug. Now, while I do not for one moment deny that such a mode of study would secure a large amount of knowledge of the Materia Medica, it seems to me suitable only for those who approach the question of homœopathy during their student's life, and before they have commenced practice. Months, if not years of study would be required before the knowledge obtained in this way would fit a man for practice, since he could only with safety employ those remedies which he had already thoroughly mastered; and unless the number of medicines learned were considerable, it would be little short of a lucky aceident if his first cases fell within the range of his therapeutic attainments.

Time will not admit of my analysing further the method Dr. Hering proposes for the conducting of such a complete study; I will therefore at once proceed to examine the plan suggested by Dr. Meyer. His leading idea is that we should endeavour to ascertain the starting-point from whence all the morbid phenomena produced by each medicine originate, and thus, having discovered the central point of attack, our knowledge of physiology will serve to explain the various symptoms radiating therefrom. Now my objection to this mode of studying is, that it is essentially wrong, and that it would in many respects lead to serious practical errors. It seems to me that its supposed advantages are founded upon the conception that the physiological sequence of phenomena is so fixed and unalterable, that a morbid impression made upon a given point must of necessity originate the same series of effects; than which nothing is less true. All experience shows that the effect is dependent quite as much upon the pre-existing condition of the person as upon the exact point from which the irritation takes its start. Were it possible to find absolutely healthy provers as subjects of experiment—persons who are not only well at the time, but who have no dormant diseases, and not even any predisposition to disease; a condition practically inconceivable—then I have little doubt but Dr. Meyer's proposition would hold true, and that one could predict with certainty the nature and sequence of morbid changes, if one was

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told the point from which they started. As matters are, however, such certainty is unattainable, and in consequence, this method of studying medicines would be worse than useless—it would be misleading. Nothing is more certain than that when a medicine is given to twenty different persons, it will display certain differences in its action, according to the previous condition of each prover; and moreover, all these differences are available in practice, because a similar series of phenomena in a patient would denote a similar condition of system, and hence would go to make up the similimum between the pathogenesis and the disease. This may be illustrated by a familiar example. The proving of a medicine resembles the giving of a shilling all round to a school of boys. The power or dynamis of the shilling is the same in all; its purchasing power is the same; but the ultimate effect will depend on each boy's propensities. One may purchase a ball and improve his health by more active exercise; another may relieve his mind by paying off an old debt which worried him; and a third may have a fit of dyspepsia by going to the confectioner's and stuffing himself with sweets. In each case the shilling was the exciting cause, while the actual symptoms depended on the predisposition of each prover.

And here I must advert to another part of Dr. Meyer's paper. He objects to teaching morbid phenomena as distinguished from diseases; by which I presume he means that we should ascertain the relations which medicines bear to concrete diseases, rather than to what Dr. Drysdale has so happily called "*elementary morbid states.*" If, however, this is his meaning, I must object to it *in toto.* My conviction is that we cannot make a greater mistake than to attempt to discover the similimum to concrete diseases in our provings; at least, if we do, we must prepare to meet with many and great disappointments, and shall, moreover, lose entirely many precious bits of useful practice. The very essence of homœopathy is that it treats *cases of disease* and not *diseases;* that it specializes so far and so minutely, that the very name of the disease is lost in the prominence given to the distinguishing symptoms of the individual case. And if this is necessary in practice, so too is it needful in the study of medicines. It is of less importance to know what medicines produce given diseases, than to know what medicines produce a

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given series of phenomena which may be easily recognised at the bedside.

This line of reasoning has led me to conclude that for practical purposes *the Homœopathic Materia Medica should be studied directly in relation to pathological states, and that the knowledge of the entire action of any medicine should be left as the ultimate result of a completed study of pathological phenomena.* I believe that in this way alone we shall be able to separate the essential from the unimportant, and that our advance will be much more secure; while at the same time a judicious division of labour might be brought to bear upon the subject, in such a manner that each person's work may be utilized by his colleagues. It is my purpose, during the remainder of the time allotted for this paper, to sketch in outline how such a study should be conducted.

1. The first requisite is to make a list of what may be termed physiological equivalents, for the purpose of enabling us to interpret symptoms and their conditions, and to determine their physiological import. This may be regarded as speculative and dangerous, and it certainly must be conducted with due caution; nevertheless, I feel satisfied that it can be done, and I have myself repeatedly felt the advantage of it. In practice we are in the daily habit of translating the symptoms observed in our patients into physiological language; and upon this we found our diagnosis. Precisely in the same way must we examine the records of our provings; and if we once succeed in forming the code of equivalents to which I have referred, we shall find this a much easier matter than it has hitherto been. To take a very simple example. We all know how important the time of day at which a symptom appears, or is aggravated, becomes in the choice between two similar and otherwise equally indicated medicines. Now so long as we seek no interpretation of such differences we must retain them in our memory as isolated facts. If, on the contrary, we bring to bear on their interpretation the investigations of M. Spring upon the diurnal variations of temperature, pulse, and respiration, we shall discover that from 3 to 9 A.M. and from 1 to 5 P.M. are periods of functional increase, while from 9 A.M. to 1 P.M. and from 6 P.M. to 3 A.M. are times of diminished functional activity.

We shall further discover that these differences are by

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no means trifling, since M. Spring has observed no less than 3½ degrees F. difference in temperature, 10-20 beats difference in the pulse, and 3-4 in the number of respirations per minute; while the exhalation of carbonic acid and of watery vapour, and also the activity of absorption, are found to vary in like manner. Applying this knowledge to our remedies, we conclude that those which present aggravations between 3 and 9 A.M., or between 1 and 5 P.M., exert their influence especially during a state of great functional activity; while those which produce symptoms or aggravations between 6 P.M. and 3 A.M., or between 9 A.M. and 1 P.M., do so by disturbing the comparative rest which the various functions of circulation, respiration and tissue change should then enjoy. Further, an enquiry into the particular symptoms aggravated, or the general action of a medicine, will, in conjunction with the above facts, shed considerable light upon the method of distinguishing between two remedies whose aggravation may occur at or about the same time. For example, *lycopodium* and *nux vom.* both have aggravations in the early morning, during a period of functional increase; but *lycopodium*, as a vegetative remedy, acts most powerfully on the nutritive changes of the body, while *nux vom.*, expending its influence in a large measure on the spinal cord, influences chiefly nervous phenomena. I have only time to give this one illustration, but others will readily suggest themselves to any one who examines into the subject; and it will be found that in this manner every addition to our physiological knowledge can be at once utilized by us in interpreting the actions of medicines. I may here refer, as a case in point, to the very instructive remarks by Dr. Drysdale on the influence of *china* and *ferrum* upon the processes connected with sanguification, by which so much light is thrown upon the action of the former in removing the consequences of loss of blood or humours, and of the latter in influencing the increase of red corpuscles, even where no chemical explanation will suffice. Not only should all "conditions" be thus analysed and interpreted, but an attempt should be made to interpret each symptom, so as to comprehend its exact physiological significance; an investigation of considerable difficulty, but yet holding out prospects of value sufficient to encourage an earnest prosecution of the work.

2. Having in this manner laid the foundation of our

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research, I would suggest the following plan of proceeding. Select some organ or tissue, and by the aids of physiology and pathology define accurately the various morbid conditions to which it is liable, and detail the symptoms, as far as possible, to which these various conditions give rise. Then examine the records of our provings, and ascertain what remedies present these symptoms in their pathogenesies; and here the student will find much of the preliminary labour done for him in the various therapeutic works which have already been published, especially in such works as those of Kafka, Hughes, and Bähr. He will there find what remedies are already known to act upon the organ or tissue that he is studying, and will thus be enabled at once to examine the best and most frequently used drugs. With pen in hand, let him note down the physiological meaning of each symptom connected with the organ in question, at first limiting his observations to the anatomical seat of the morbid change. Then going over the ground a second time, let him note the kind of action elicited by the drug; whether it be a mere increase or diminution of its natural function, or whether that function is perverted, so that a result qualitatively different from what it should be has occurred; and further, in the case of such perverted function, the course and extent of the change should be noted. Another examination should then be commenced for the purpose of noting especially the conditions and concomitants of all these changes. For the purpose of comparing different medicines together, it is very necessary that all the particulars herein referred to should be noted in a given order, such order having been determined upon when drawing up the physiological and pathological schema of the organ.

I will endeavour now to illustrate this by an example. Let us suppose the student to be engaged in studying the various diseases of the mucous membrane. His first step will be to obtain a correct knowledge of the anatomy, physiology and pathology of this membrane, and especially he should note the differences observed in the tendencies to disease and in the order of pathological changes in the different sections of this membrane. Having arranged a schema embodying the knowledge thus acquired, we will suppose he takes up the morbid condition known as catarrh, and first endeavours to ascertain what medicines stand in specific relation with this disease as occurring in

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the naso-pulmonary mucous membrane. 1. He will first arrange them according to the parts of this tract which they are known to affect, and he will find that *euphrasia* acts especially on the mucous membranes of the *eyes and nose*, while *allium cepa* acts on the *nose and eyes*. The order is here reversed, because the action of *euphrasia* appears to commence in the conjunctiva, and certainly attains a higher degree of intensity there, while *allium cepa* exerts a more decidedly irritant effect on the Schneiderian membrane and acts much less powerfully on the conjunctiva. In proof of which we find in *euphrasia* acrid tears and bland nasal mucus, while in *alliun cepa* the tears are bland and the nasal mucus is acrid and irritating.

Besides these, the student will find *arsenic* acting pretty equally on both eyes and nose, and extending its effects to the larynx and trachea; *mercurius* causing symptoms of acute nasal catarrh, while its conjunctival symptoms are more related to the subacute and chronic inflammations of that membrane; *iodine* and *iodide of potassium* acting powerfully on the nose, but extending its influence well down into the larynx; and *nux vom.* interfering with the secretion of the nasal mucous membrane, while apparently exerting but little influence on the state of the membrane itself. He will ascertain further that *rumex crispus, nitric acid, ipecac., ammonia, bromine, antimony, spongia, hepar, caust., kali bichr.*, and many others irritate in different degrees the larynx and trachea. Again, *bryonia, squilla, phos.* and some others seem to commence their irritation in the *trachea and larger bronchi*, and some of these, as *kali bichr., merc. corr., ant. tart.*, and *phos.* and *ammon. carb.* extend their influence to the smaller bronchial tubes, while two of them, viz., *ant. tart.* and *phos.*, do not limit their irritation to the bronchial ramifications, but act upon the parenchyma of the lung itself.

2. Looking now to the kind of action, the student will find by reference to his physiological schema that mucous membranes are liable to the following changes:—The membrane may become simply dry, owing to exhalation becoming checked; or it may be dry and irritable, owing to its nerves being implicated in the change; or it may be dry and red, owing to its receiving an abnormal supply of blood—which increase may depend either upon simple relaxation of small arteries consequent upon vaso-motor

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depression, or by overfilling of the capillaries caused by tissue attraction; or, finally, it may be dry and swollen, owing to effusion of serum into the connective tissue. On the other hand, the mucous membrane may be over-moist, and this moisture may be simple excess of watery exhalation—a condition analogous to sweating; or excess of mucus, owing to the epithelial scales being set free while still retaining their primitive rounded form, in which they are undistinguishable from mucous corpuscles; or effusion of serum from over-distended capillaries; or sero-mucus from a combination of the two last conditions; or ichorous where interstitial waste of tissue is excessive; or muco-pus from fatty degeneration of mucous corpuscles; or viscid adhesive mucus from a certain admixture of fibrinoid matter in the excretion. Again, the surface of the membrane may be encrusted with exudations too solid to be washed away, and these encrustations may be simply coagulated lymph, as in membranous croup; or modified epithelium, as in membranous dysmenorrhœa; or possessing a character *sui generis*, as in diphtheria; or, finally, the surface of the mucous membrane may have been destroyed, giving rise to all the known varieties of ulceration, from simple excoriation to phagedæna and sphacelus.

Possessing a correct knowledge of all these varieties, he will then allocate his several medicines according to the condition of membrane which he finds they have produced. I need scarcely mention that several of these conditions of mucous membrane will often be found in one proving. But this will form no practical difficulty, since it will always depend upon one of two things; either the different conditions are in reality different stages of one and the same pathological process, or the remedy is capable of producing these different conditions in persons having different pathological tendencies (dormant diseases); and as I have already remarked, if it can produce these varied conditions, it can also cure them. For example, dryness of mucous membrane will be found under *alum*, *benzoic acid*, *iodine*, *nux v.*, *spong.*, *sticta*, &c. Dryness with irritation under *laches.*, *nitr. acid*, *rumex crispus*, *senega*, &c. Dryness and redness in *bell.*, *bry.*, *ars.*, *phos.*, &c. Dryness and œdema in *apis*. Increased moisture from active exhalation is found under *sambucus*, *dulc.*, and *ammon. carb.* Excess of mucus under *ammon. mur.*, *ant. c.*, *squill.*, *merc.*, &c. Serous discharge under *solania* and

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veratrum. Sero-mucus under *ammon. carb.* Ichor under *ars.* Muco-pus under *pulsatilla, stannum, hep. s., ant. tart., sulph.,* and many others. Viscid and fibrinoid under *kali bichr.* Exudations on the surface will be found in *bryon., iod., bromine, kal. bichr., spong.,* &c.; and ulcerations occur in *phos., hep. s., silic., calc.. lyc.,* &c.

It must be understood that in this example I am only referring to a few medicines under each heading, and am not attempting to exhaust the list of remedies suitable for each condition.

When this much is accomplished, the student may compare his lists together, and he will at once perceive that when he endeavours to associate locality of action and kind of effect together, he will split up his list of remedies into very small groups indeed, or in other words, these two items alone, viz., similarity of seat and similarity of action, will serve to separate most medicines from one another. I must, however, give a caution here, lest an over-preciseness should lead to subdivision beyond what is prudent. Both as regards locality and kind of action, we must not rigidly define the limits of either, unless the medicine has been fully and exhaustively proved; since the fact of a medicine having acted in one or two provings upon one part only of the mucous membrane by no means demonstrates that it will never act upon other portions. Or again, the fact of a thin, watery exhalation having been found to follow the taking of a certain drug by no means excludes the possibility of its causing a flow of mucus or muco-pus in other cases. Except in very well proved remedies, it will be perfectly legitimate to conclude that medicines displaying a marked affinity for the mucous membranes will in all probability act on any part thereof where there is either similarity of function or of structure. On this account it is well to group together all those parts of the mucous membranes which possess exactly the same anatomical structure, distinguishing the various forms of epithelium and the presence of many or few layers of cells; distinguishing also with great care between simply exhaling portions and those which secrete mucus, and those other portions which yield varied and special secretions; and yet once more, distinguishing those parts which have special nervous endowments, such as the peculiar susceptibility of the conjunctival and laryngeal portions to the irritation caused by any foreign body.

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I am much afraid that many will say that the plan of study which I am suggesting is quite as complex as the Materia Medica itself; but such is not the case. Every item of knowledge gained in such a study as this is a direct step in advance, since it can be utilized not for one medicine only, as when we learn a symptom by rote, but for all medicines; not for one disease only, but for all diseases. What is the plan, indeed, but a careful study of minute physiology, and the applying of its results to the elucidation of the action of medicines. It is my intention, if I can command the necessary time, to apply this method to the whole subject of catarrh, following it through the whole tract of mucous membrane, and comparing together the actions of all our hitherto proved remedies.

Another very important point to be studied is the interdependence of symptoms. A careful application of physiological knowledge to the interpretation of morbid phenomena will demonstrate that certain changes of function must of necessity produce certain other changes, so that if one link of any well-known chain is found, the others may be predicated with certainty. In this way we are enabled to build up an entire morbid picture, much in the same way as a paleontologist builds up an extinct animal from a careful examination of a single bone. The assistance obtained in this way in the selection of remedies for disease is very great. For example, nothing is more common than such a case as the following:—A patient complains of certain symptoms which indicate in a marked degree congestion of the pelvic veins, and yet among the symptoms there exist some which are not found in the provings of *sepia*, or *collinsonia*, or *aloes*, or any other of the remedies known to produce pelvic congestion. What then is the homœopathist to do? Is he to examine the case purely symptomatically, or is he to prescribe one of the above remedies, notwithstanding the absence of the symptoms referred to? This will depend entirely upon the following circumstance. If the peculiar symptoms are of such a nature that it is physiologically evident that they are caused by pelvic congestion, I believe he may with safety prescribe according to the general indications, and feel confident that these symptoms will subside with the removal of the originating cause. If, on the other hand, no necessary connection can be traced between the pecu-

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liar symptoms and the chief morbid condition, then I believe it will be safer to prescribe for the case symptomatically, and, if an exact similimum to the peculiar symptoms can be found, to give such a remedy in place of one known to produce pelvic congestion, but not possessing the symptoms in question. Such a proceeding will probably be characterised as unscientific and mechanical; but it in no way merits the name. It is, in point of fact, neither more nor less than a confession of limited knowledge. If our provings were perfect, and our acquaintance with physiology co-extensive with the laws of vitality, then indeed such a plan would be needless; but until that happy day arrives, we must content ourselves with using helps of all kinds, whenever the exigencies of practice require us to go beyond the limits of our perfected investigations. We do not know as yet the entire action of any one medicine; and hence it is perfectly possible that, in the second case, the medicine selected symptomatically might, if fully proved, show evidence of its power to cause pelvic congestion, just as in the first case the remedy which caused pelvic congestion and had not the peculiar symptoms, might have manifested these had the proving been more fully carried out. We cannot too carefully bear this in mind; for otherwise our endeavours to prescribe physiologically will become fraught with danger just as great as that which besets the path of the blind symptom-hunter.

I know it will be objected by some to this method of study, that no single medicine will be learned thoroughly until the whole Materia Medica has been mastered. This, however, is only true theoretically; since the proposed plan is such that it admits of being carried out in various degrees of completeness, and yet each degree will possess its full value. Thus, without much difficulty, one could arrange all the well-proved remedies, both according to their seat and kind of action, from the material ready to our hands in the therapeutic works already published; and thus a good and useful general idea of the action of all the ordinary remedies could be obtained in a short time, and the student would then be enabled to utilize such knowledge at the bedside. As he advanced with his studies he would find each additional piece of work would fall into its place at once and become practically valuable, and his position would thus be vastly superior to the stu-

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dent who, following Dr. Hering's advice, had devoted his time to the thorough mastery of a single remedy, and then used it as a ground of comparison for all other medicines. A much more serious objection is, that owing to the imperfection of our knowledge of physiology, a considerable number of symptoms in every proving would be "left out in the cold," no place being found for them in the best constructed physiological schema. And it becomes a serious question how to deal with this residuum. To neglect it would be most unwise, since many valuable bits of practice will be found among these unmanageable items. For my own part, I believe we must just accept the difficulty, hoping for the time when the light of advancing science will clear up all these obscurities; and must apply them symptomatically, whenever a suitable opportunity offers. I have already referred to this subject more in detail, in a paper on the proper place of repertories in homœopathic practice, which was read before the British Homœopathic Society during last session, and which will be published in the October number of the *British Journal of Homœopathy*. Some of our work we must do mechanically, and to that extent we must feel that we are walking with crutches; but we may nevertheless proceed cheerfully, in the full conviction that every advance in physiology will help to explain away our difficulties; and if, in our regular study, we have kept abreast with the vanguard, we may feel confident of utilizing every new discovery as soon as made, and indeed long before the discoverer himself, if not a homœopath, can perceive any practical utility in his work.

I have thus endeavoured, I fear very imperfectly, to set before you what, after more than twenty-five years of thought and endeavour, I believe to be the most practicable and practically useful method of studying our Materia Medica. I do not claim for it any title to novelty, and far less do I consider it perfect; but nevertheless I believe it to be the best which has hitherto been suggested, and if carried out by the joint labours of many, would yield a Manual of Therapeutics far superior to any we at present possess. Trusting that some of my colleagues will take up the matter and help the good work forward, I leave these remarks to their careful consideration, and shall be only too happy to receive their friendly criticism.

36, Sackville Street.
September 1870.

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ON SYMPTOMATOLOGY.

By Thomas Hayle, M.D.

Perhaps there is nothing more remarkable about the human organism in a healthy state than the absence of symptoms; and by this I mean the very little appeal it makes to the consciousness with which it is connected. It is an old saying that "a healthy man shouldn't know that he has a stomach;" and it would be the perfection of an organism, if the consciousness connected with it shouldn't know it had one, except through the senses by which it has in that organism a relation to the external world.

To this perfection a perfectly healthy organised system very nearly attains. Let us consider what this absence of conscious feeling involves. It involves, first, insensibility to countless torrents of blood, rushing through countless channels through every part of the organism, these channels interwoven so intimately, and so minutely divided, as to form a tissue composed of threads invisible to the naked eye; then it involves insensibility to the play of a very elaborate and complicated locomotive apparatus, muscle and tendon, joint and bone, in its perfection putting to shame the utmost efforts of human ingenuity. Going deeper, it involves insensibility to the intimacies of chemical affinity by which the whole structure is continually being disintegrated and renewed, the flow of electric, galvanic and magnetic currents, and the play of that exquisite, at present inexplicable, and perhaps ever unimaginable apparatus through which thought and feeling come into manifestation. Fearfully and wonderfully made, truly! but more fearful and more wondrous still, the insensibility and the unconsciousness that sit enthroned in the midst of so much movement and in the midst of so much feeling!

Now this does not result from consciousness being in a

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distinct sphere from that in which these complex operations are going on, so as to have no bond of connection with it. It controls some of these operations by acts of its will, and has the means of ascertaining that its commands are being carried out; and in all of them, when things go wrong, it has soon information of the fact, some few cases excepted. The mode in which this information is conveyed, varies with the tissue affected and the kind of operations that are disturbed. Subjectively or objectively, or in both ways, the disturbance makes itself known in language unmistakable, could we but read it.

I do not mean to assert that this is a language which we are altogether unable to decipher; but yet I do not consider pathology, or the science of the interpretation of this language, in as advanced a state or as near perfection as it is often spoken of as being. It is true that great strides have been made in this direction of late years. Increased accuracy of observation, improved and newly-invented instruments for investigation, a greater knowledge of chemistry and of the finer forces of nature may have increased our powers, enlarged our field, and enabled us to analyse with microscopic minuteness the corpuscular changes in a tissue; but after all is said, it must be confessed that we generally have to deal with a *caput mortuum*—with results which take their place with the strata of geology, throwing indeed much less light than these on the causes which produced them, belonging as they do to a realm less known, more recondite and mysterious, perhaps altogether inscrutable.

Take, for instance, a part in a state of inflammation. We know a great deal about this pathological state. It is one of most frequent occurrence, and makes its existence known by unmistakable signs; both microscopically and chemically it has been most minutely examined, so that we may be fairly said to be molecularly acquainted with it. We can describe its commencement, its progress, and its terminations, so as to present in minutest detail the sequences of its history.

And yet how little do we know of its immediate antecedents and its remote causes? Why does it happen more in one individual than another, and in one spot or tissue more than another? and why do different kinds occur in the same tissue? Why, for instance, do we sometimes see on the skin punctuate or flea-bite inflammation, and some-

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times diffuse inflammation like that of erysipelas or scarlatina? And why does the same agent produce sometimes one kind of inflammation and sometimes another, as it acts on this or that individual; as, for instance, such medicines as *rhus* or *sulphur?*

Plausible, perhaps correct answers, may possibly be given to some of these questions; but enough has been said, I think, to show the obscurity in which the subject is involved, and to convince us that inflammation is the product in each case of a number of unknown factors, which in the present state of our knowledge are utterly undiscoverable.

It becomes, however, the duty of every philosophical inquirer to observe and note the antecedents and concomitants of every such phenomenon, whether they be external to the individual or contained within his organism, so that we may be enabled inductively to ascertain what are the constant and therefore necessary accompaniments of the state, and this to a much greater degree of minuteness than is even dreamt of by the ordinary observers of disease—certainly, than is practised by the greater number of the disciples of homœopathy.

In this investigation it becomes of the greatest importance to estimate the pathological value of each symptom, however minute and however apparently trivial. In what tissue does it arise? from what state of that tissue? and out of what general or constitutional state? In this direction of inquiry I know no one who has been more systematic and logical, or more acute, than Dr. Hirschel of Dresden, whose appreciation of minute symptomatology is the most valuable contribution that has appeared, to the scientific study of disease, whether sporadic or induced. Some years ago I endeavoured to give some idea—a very inadequate one, I fear—of his work, the *Grundriss*, by translating selections from it. I believe, with the exception of the polemical portions, these selections comprised the essential points of the work. I do not think there is any work in English equal in practical value to Dr. Hirschel's *Arzneischatz*, which I also translated, but was unable to find a publisher. His *Magenschmerzen* is also a work of great value, and is well worth translation, if only as a model. But to return.

It is evident that it is only in this way that we can attain to a scientific appreciation of symptoms, and learn

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how to affix a meaning to each distinct expression in the language of the diseased organism.

As we proceed in this study our interest will increase, as well as our confidence in the correctness of our explanations. The more deep our penetration into the meaning of each symptom, the more clearly shall we be able to see the convergence of each to one general causal state—just as the convergent flight of two bees directs the beehunter to the hive—and each will be seen to arise out of one and the same general state, of the existence of which our evidence will thus be complete.

We may expect, the further we push our inquiries, to attain increasing minuteness and exactitude in our knowledge. Each case will become individualised, and instead of being treated generically will demand and obtain special treatment. This must necessitate a special choice of remedies; and practitioners must ultimately be brought face to face with the great homœopathic law of cure, as expressing a fixed relation between the disease and the remedy—face to face with a law, which no flank movement of using homœopathic remedies, while ignoring the principle of their action, will enable them any longer to evade. A most brilliant illustration of what may be expected in this direction is Dr. Reith's adoption of homœopathy, which, though merely a first fruit, is not likely soon to be paralleled, whether we consider the sagacity and honest fearlessness of the inquirer, or the piquant history of the effect of the gradual development of Dr. Reith's views on the medical world. It puts me in mind of the balloon party, who at first highly delighted with the enlarged views they were getting, began frantically to pull at the valve, when they found they were being floated in the direction of a highly improper territory.

But further; not only will increased precision and minuteness in our knowledge of the phenomena of disease and of their causes disclose to us the mode of action of remedies, and thus the law of cure, but it will give scientific precision to our application of the remedies themselves, and thus facilitate the practice of homœopathy.

I am speaking in the presence of men to whom the difficulty of this practice is a matter of daily experience. Difficulty is too mild a term; in the present state of our knowledge it is but too often impossible to find a true simile. So much is this the case, that the similarity upon

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which we are often compelled to act is more an analogy than a similarity; sometimes we act merely on a knowledge that the medicine chosen acts somehow on the organ affected. We must put up with action on the part affected, not knowing whether the action induced will be similar to the existing action or not.

Perhaps nothing calls the attention more to the existence of this rough-and-ready mode of homœopathising than what happens in a consultation; for that which is apt to pass unnoticed in the usual daily routine of solitary practice is in this case made objective. Well, what do we find? There is, to begin with, a thoroughly scientific and searching examination of the patient; all that relates to the diagnosis is accurate and precise; the stage of the disease and the constitution of the patient are agreed on; but when we come to the choice of the medicine all this precision leaves us. It is ascertained that the ordinary remedies have been administered, most likely alternately, sometimes without definite order or any very rigid examination of their homœopathicity to the special case in hand, probably on generic rather than on specific resemblance; and the remedy finally agreed on may be chosen on analogies equally loose—may, perhaps, never have been proved.

I myself do not come into court with clean hands; and every word I have said hits some memory of my past.

The Germans are a very thoughtful people, and I don't think there is as much "schlendrian," as they call it, among them as among us; and some of them, I know, repudiate our modes of alternation. Dr. Bähr, indeed, has said somewhere that it makes our treatment of disease valueless to those who read our cases.

Yet even among them the difficulties of applying the homœopathic principle in practice have been so keenly felt as to lead some to the verge of scepticism, as instanced in Kurz of Dessau's article, the chief value of which, exclusive of its honesty, was in calling out the truly valuable, clear-headed and logical utterances of the lamented Trinks and his friend Hirschel. These scientific exponents of the facts of the case pointed out that in our Materia Medica Pura we had a treasury of experimental knowledge as to the real actions of drugs on the human organism, which, through the relation expressed by the law of similars, became available for the treatment of

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disease, and which, when repeatedly verified by clinical experiment, took rank among the positive objective truths of science.

Let those, then, who complain of the mass of rubbish which our Materia Medica contains take comfort. In clinical experiment we have the means of separating the wheat from the chaff, the gold from the dross, and while we *ipso facto* relieve the sufferings of humanity, facilitate indirectly that relief for all coming time. It is a beautiful analytical method, this *usus in morbis;* it has rare advantages. By it we can place side by side two morbid processes, with a certainty that they are similar. We are assured of that by the spark of their mutual extinguishment. We can compare them, and make the salient phenomena of the one throw light on the latent processes of the other. We can investigate the mysteries of the unknown by the action of an agent which is known, and by instituting artificial diseases under our own conditions, discover the nature of those which are sporadic. I may add, that the subjective symptoms of the proving brought to bear on those of the case in hand, and extinguishing them, assume an objective value, in some sense, at least. I look for a continuation of Dr. Madden's researches in this direction with great interest. Much light has already been thrown upon many ill-understood and obscure points connected with the homœopathic law by the researches of our respected President, and much more may we not hope from his great industry, extensive reading, and clear, acute mind?

What a rich harvest of general laws will there be for some Kepler of a coming age and some following Newton to reap, when scientifically-conducted provings of medicines, together with careful, accurate and minute histories of the diseased conditions which these remedies have checked or removed shall exist in sufficient number!

Hahnemann had justly a great distrust of the pathology of his day, consisting as it did mainly of a tissue of hypotheses, and with the practical sagacity of the true man of science, insisted on resting similarity on the totality of the symptoms. This was proceeding purely on observation, he thought, and was a mode involving no hypothesis. For a season homœopathists followed this symptom-covering plan with the most painstaking and Chinese scrupulosity; and we hear of such things as "a slipperi-

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ness or smoothness of the teeth with much mucus, worse for two days after shaving," (I suppose the gentleman didn't shave often,) determining the choice of the medicine, which it may be interesting to know was *carbo animalis.* Practical and thoughtful men could not, however, long be tied down to this irksome and profitless mode of practice. Pathology began to be studied scientifically, and was applied to medicinal as well as to sporadic diseases, and practitioners began to look more for general resemblance in the important pathognomic symptoms, and became comparatively careless of minute correspondence. The manuals of Drs. Hempel and Hale of America, and of Dr. Hughes of Brighton, are written in this spirit, and commend themselves, I suppose, to the common sense of the great majority of homœopathists at the present time. That a very considerable amount of success, especially in acute disease, is attained by those who act on these principles, I know as a fact, and we may be sure of, when we consider the character and practical sagacity of the men who act on them; but I feel assured they do not give us by any means the full value of homœopathy. I have spoken above of our utter ignorance, in almost all cases, of what may be called the internal history of an objective fact such as inflammation. It is like a great war. A war is an objective fact; but the hidden springs of it, the diplomatic intrigues and manœuvres which have led to it, are far below the surface, and most difficult to get at. So is it with an inflammation. Each inflammation has its pathognomonic signs by which it is known, and in these, inflammations of each kind are alike, but differ in their internal antecedents, each in kind and degree, to an extent we have no means of ascertaining.

Now where pathology in its present state fails as a guide, homœopathy steps in. There are often symptoms which we cannot connect causally or pathologically with the inflammation in hand, but which occur in the proving of a medicine known to produce the same kind of inflammation. We thus are enabled to separate inflammations into different kinds, without knowing the causes of the differences which distinguish them, and treat each more successfully by the administration of the specific than by that of what may be called the generic remedy.

The law of similarity has passed beyond the limits of our pathological knowledge, and has been a guide to us

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where we could no longer see our way. The case is vastly stronger when we come to chronic disease, with its almost infinite variety of symptoms. Here we have a labyrinth and a riddle which sets at nought the utmost efforts of our analytical faculties. It certainly is a little better than total ignorance to know in these cases the generic actions of remedies, the organs they act on, and the more prominent of their modes of action; but the treatment of chronic disease would be a sorry business, if we were deprived of the minute symptoms occurring in the experiences of our self-sacrificing and painstaking provers.

I suppose it is the custom of most of us to prescribe in general without reference to our books; but how often must it have happened in the experience of each of us, that having failed in the treatment, and being driven to our books and a more careful search for a correspondence of symptoms, we have met with an immediate and surprising success, owing sometimes to a minute similarity in symptoms, of which we could not see the pathological bearing. I have just met with a case in point, in a man of middle age: Pain like *stabs* with a knife, or shoots, across sacrum, worse on stooping or rising from seat, no pain when recumbent; pulse 90; a week attacked. *Verat. alb.* 30, a pilule every four hours. For six hours frequency of stabs increased, severity diminished; then total cessation. Now, four days after, only feels weak. Felt the medicine go to the part directly. Neither Hempel nor Hughes allude to this action of *veratrum* at all. Nor should I have thought of it but for Ruckert. I may add, that on a return of the symptom I failed with *veratrum* in any attenuation; aggravated the symptom and upset the stomach by a very low attenuation of *phosphorus*, but succeeded immediately and permanently by *phosphorus* 30, to the choice of which remedy I was again led by Ruckert.

To my mind, Gross's *Comparative Materia Medica* is an unsatisfactory book; but I can conceive a work carried out on that plan, in which the differences between the medicines should be interpreted pathologically, as far as we could go, being of very great use. As it is, I must confess some obligation to that painstaking and ingenious work.

In conclusion, while I deplore the neglect of the rea-

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soning faculty that has been shown in mere symptom covering, I equally repudiate its abuse when every similarity is rejected which cannot be explained. I confess the unsatisfactory state of a very large portion of the Materia Medica, but protest against its rejection, inasmuch as we have the verification of symptoms *ex usu in morbis* to fall back upon. I admit that the recoil from the irksome and often frivolous and absurd minutiæ of the early homœopathists to the easier but often rather loose practice of the present day was natural, and in some respects rational, just as the recoil in English high life from Puritanism to licentiousness was so; yet I maintain that, as in the one case a moral and religious order equally removed from Puritanism and licentiousness is absolutely necessary for health and happiness, so in the other, that there exists a scientific practice equally removed from a slavish and irrational symptom-covering and from a licentious "schlendrian," which will and does illustrate and confirm the law discovered by Hahnemann, to the satisfaction of all conscientious and competent inquirers. I feel therefore persuaded that there will yet be a return from the present rather too prevalent use of canister and grape shot—fired often without order, and though generally in the direction of, yet often without special aim at the disease—to the patient, deliberate and skilled use of the needle-gun, with new sights, of the sagacious old German of blessed memory.

Rochdale, September 1870.

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ON ACUTE RHEUMATISM.

By Francis Black, M.D.

The medical history of acute rheumatism is an epitome of the general history of medicine, and it illustrates the different and often changing views which for centuries have overrun the healing art. But though a subject interesting in itself, it is far too extensive for the time of this Congress, where it is so desirable to place the points for consideration, and discussion in the briefest, and most distinct form. I shall therefore limit my remarks and your discussion to the question, *What influence has homœopathic treatment on the course of acute rheumatism?*

By acute rheumatism I allude to a disease characterised by febrile excitement, and an inflammation of a peculiar kind, attacking the joints and the white fibrous tissues, accompanied by profuse excretions from the skin, generally of an acid character. This definition is sufficient, without entering on the metamorphic conditions which are present.

In order to illustrate my argument, I shall now briefly narrate a few cases, treated homœopathically:—

An officer of the Indian army, stout, tall, and of lymphatic temperament, had been ailing for some days previously to the 25th September, 1868, when he felt aching of the elbow and wrist. On the 29th, when I first saw him, the pain had much increased, and the affected parts were covered with patches of erythema nodosum; he had also pain in the knee, with slight erythematous appearance; he complains much of malaise; the tongue is furred; the skin is hot, and the pulse is 86. He has suffered from cardiac affection, following rheumatic fever, since he was 15; the heart is slightly hypertrophied,

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and the aortic valve impaired. *Rhus,* 1st dec., one drop every three hours.

Sept. 30th. The eruption increased. Cont. *rhus.*

Oct. 1st. The erythema is much less, but the rheumatic pains are more acute. *Bry.* 1, one drop every two hours.

2nd. No change. Cont. *bry.*

3rd. He has passed a restless night; the perspiration comes and goes; the pains in legs are better, but he complains of acute pain in the chest; the heart's action is increased and tumultuous, the systolic bruit harsher, there is also slight pericardial friction murmur; pulse 92. His look is very anxious. *Acon.* 1 every hour.

4th. Slept better; the cardiac uneasiness much less; pulse 90; heart's action steadier; perspiration constant. Cont. *acon.* 1.

5th. Thoracic pain and cardiac uneasiness nearly gone; no pericardial sound, and the systolic bruit less loud; the heart's action steady; new patches of erythema. *Rhus,* 1st dec.

6th. He has exposed himself during the night, and there is a return of acute cardiac pain, with hard action of the heart; pulse 105; the breathing is oppressed; there is wakefulness and great restlessness; the countenance is anxious. *Acon.,* 1st dec., every half-hour until easier.

7th. Speedy relief after a few doses; cardiac pain much less, the action more natural; the pulse is 100; he has slept a little. Cont. *acon.*

8th. Slept well; cardiac pain gone; pulse 96; heart's action quieter; bruit as before; pulse 96. The rheumatic pains are now confined principally to one ankle, where the swelling extends down the instep. The urine very scanty, and loaded with lithates; acid perspiration lessening. *Colch.,* 1st dec., every three hours.

9th. He is better in all respects. Cont. *colch.*

On the 10th the pulse was 84, and on the 11th still further improvement. Cont. *colch.* But on leaving his bed on the 12th, contrary to orders, he suffered from an increase of rheumatic pains. Cont. *colch.*

13th. Rheumatism much less; hardly any perspiration; he has great uneasiness at the heart, with fainty feelings; the action is feeble, the bruit as formerly; the pulse 96, is weak and intermitting. *Dig.,* 1st dec., two drops every two to three hours.

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14th. Better. Cont. *dig.* On the 16th all cardiac uneasiness had ceased; the pulse normal, but the urine had become more scanty and loaded with lithates; the ankle and knee more painful. *Colch.*, 1st dec.

16th. Much better; but on the 17th, after exposure to a draught, he was attacked by acute pain in cardiac region, attended by great uneasiness; the pulse 100. *Spig.* 1 every half-hour until better.

18th. The pain is much less; pulse 96. The improvement steadily progressed up to the 23rd, when I considered him well; up to this date *spig.* 1 was given. *Arsenicum*, 3rd dec., three drops four times a day, was administered for a fortnight, with a view to benefit the heart. At the end of this period the heart had gained power, exercise excited less palpitation than before the attack, and the general health was much improved.

This patient was not placed between blankets; the local treatment consisted in the application of wet compresses to the affected joints. The diet was strictly farinaceous, up to convalescence.

Case II.—A lady, aged 26, was in Nov. 1868 confined of her third child. The recovery was very tedious, owing to a severe attack of phlegmasia alba dolens, and pleuritis. For these ailments she was treated in the ordinary way, and did not come under my care until Feb. 26th, 1870, when she was still weak. She had for some days been feeling a general malaise, and now for twenty-four hours she has suffered from acute pains in the back, chest, and left arm. During the 27th these increased and prevented movement. The pulse is quick; the cardiac sounds normal; the skin covered with profuse perspiration; the tongue is furred and the throat is very red and painful; the bowels are costive and the urine is scanty and high coloured. The patient is to lie between blankets; to take *actea*, 1st dec., one drop every two hours. The diet to be farinaceous, with a little milk.

28th. No improvement. Pulse 96; heart normal; the pains and swelling involve both knees. *Bry.* 1, one drop every two to three hours; wet compresses to the affected parts. This was continued with some relief to the pains, so that sleep was procured; but on the 1st March the catamenia appeared, and to relieve the general disturbance caused by this, *puls.* 1 was given, and continued to the

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8th. By this time the pains and swellings were much less; the pulse was normal; the tongue was clearing; the urine had become abundant and clear, when she was suddenly seized with acute pains all through the chest; no abnormal sounds. *Bry.* 1 from every half-hour to hour, according to the relief, which was soon afforded. On the 11th the patient was convalescent, and able to leave her room.

Case III.—This lady continued well up to the 26th, when she began to suffer from sore throat, headache, hot burning skin, and pulse 100. I feared scarlatina, as her child had it. *Bell.* 1.

Mar. 27th. The throat less painful, but swollen, and of a bright red, which extended to the palate; no rash on any part of the body. She complains of acute pain and tenderness of the chest and left shoulders, and down the back; pulse 110. *Bry.* 1 every two hours.

28th. Pulse 110; she is restless, even though movement excites so much pain, which now involves the knee; the throat is nearly well. *Acon.* 1 was steadily continued until April 1st, when the pains had almost ceased, being limited in a slight degree to the left shoulder. *Bry.* 1.

April 2nd. Without any assignable cause, there is an attack of sharp pain from the occiput down the spine, with inability to move the neck; pulse 80. *Actea,* 1st dec., every two hours. This was continued with relief until the 5th, when for intolerable itching of the skin, all over the body, *rhus* 1 was administered, and continued until the 9th, when she was able to leave her bed.

13th. No rheumatic pains; desquamation is commencing, and continued for a week. The attack was evidently one of scarlatina combined with rheumatic fever.

Case IV.—A widow lady, aged 54, stout, and of a bilious temperament, and who has long been dyspeptic, was attacked by acute rheumatism Feb. 24th, 1870. The pains were sharp, and affected principally the back and loins, so as to prevent motion; they then extended to the left arm, afterwards to the knees and ankles. During nearly the entire course of the attack there were profuse perspirations. At first the pulse ranged about 80, and rose to 90 and to 100. The digestive functions were much affected; the tongue very furred; a constant painful dry-

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ness of the throat, not relieved by drinking; tenderness on pressure over the liver; the bowels constipated; the urine scanty and high coloured.

Bry. 1 was given during the 25th, 26th, and 27th, but with no relief to the pains; then *actea* was prescribed for three days. On the 3rd March the rheumatic pains and swellings were less, but there was great distress from the condition of the mouth and throat, which were swollen, as also the tongue, and covered thickly with a greyish white offensive exudation; the parotids were swollen. This exudation was very like diphtheria, but I was inclined to regard it as of the nature of severe aphthous stomatitis. *Merc.* and then *bell.* were given for four days. No improvement. Then, March 8th, *nit. ac.*, 1st dec., was administered in frequent doses, and soon the exudation, and swelling diminished, and on the 10th they had disappeared.

The attention was now directed to the rheumatic pains, which shifted much from one joint to another, but were constant in the left arm, and right side of the neck. *Puls.* 1.

Mar. 14th. Joint pains much less, but she complains of acute pain in the hepatic region, and through to the back; pulse 90; bowels costive. The patient begged to have a sedlitz powder; two were given, but with no effect on the bowels; then two grains of *podophylline*, 1st dec., were administered, and soon there was a free action of the bowels, and relief to the bilious symptoms.*

20th. Much better as regards the rheumatism, and bilious symptoms, but there is considerable dyspnœa when she moves or sits up, which she now does for the first time. The heart is normal, as it has been all through the attack, but there is effusion occupying two-thirds of the right pleura. *Sul.*, 3rd dec., was given up to the 26th, when the effusion had much diminished, and on the 31st the patient was able to take a long railway journey.

Case V.—A strumous child, aged 8, just recovering from ulceration of the cornea, and suffering from eczema of the face and ears, was affected on the 12th February, 1870, with badly developed scarlatina. The attack was a mild

* The inability to move, the supine posture, and the objection of the patient prevented enemata being tried. She had previous to this illness taken aperients daily; hence the reason for this departure from purely homœopathic treatment.

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one. On the 13th Feb. she complained of feverishness, pain and swelling of the wrists and elbows; soon the knees and feet were involved. The pains were worse at night; slight perspirations; urine scanty, high coloured, and albuminous. *Merc. corr.*, 3rd dec., was given until the albumen disappeared, and on the 19th, when the pains were much less, *bry.* 1 was administered, and continued until the 23rd, when the patient was quite well.

These five cases which I have briefly reported are selected, not because the patients recovered, but because the disease was well marked, and affected subjects and under circumstances where the history of acute rheumatism leads us to expect that the attack would be severe and protracted. In the first case the patient is a man who has lived freely, who is in a bad state of health, and who has been the subject of heart disease since youth; and yet the duration of the disease was 24 days, and the cardiac complication quickly passed away.

The second patient is a delicate lady, exhausted by a difficult confinement, and barely convalescent from white leg and pleurisy, when she is attacked by severe acute rheumatism: this ceases in 13 days. She is convalescent a week when she is attacked by scarlatina and another (third case) severe seizure of acute rheumatism; and from these she recovers in 12 days.

The fourth case is not an instance of pure homœopathic treatment, because the patient, owing to peculiar circumstances, was allowed to take an aperient powder and two doses of *podophylline;* but with those exceptions the remedies were homœopathic. The attack was severe, and though complicated with aphthous stomatitis, and pleurisy, lasted 34 days.

The fifth case is a delicate strumous child, who has acute rheumatism and albuminuria as a consequence of scarlatina: she recovers in 8 days.

Such results, and they correspond with my general experience of the disease during more than a quarter of a century, confirm the views I held in a paper which I published in the *British Journal of Homœopathy* in 1853. I was then led to investigate the question, stimulated by the supposed unfavourable or rather negative results of Drs. Wurmb and Caspar in the Homœopathic Hospital in Vienna. I then shewed that the duration of

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their cases approached very closely to, but was generally less than that recorded by Chomel and by Dr. Fuller. I also judged from my own experience and that of colleagues, that the average duration, including complicated cases, was from 19 to 20 days. My experience since 1853 confirms the view I then held, that homœopathic treatment is superior to the ordinary treatment of acute rheumatism.

As shewing the duration of acute rheumatism under ordinary treatment, let me quote Dr. Fuller:—*

"The duration of an ordinary uncomplicated attack of acute rheumatism has been variously stated by different writers. Dr. Warren reported six weeks as its ordinary duration; Sir Charles Scudamore stated that, 'in a case of which the issue is favourable, the fever and pains are brought to a stand *at the end of the third week*, and in slighter attacks at an earlier period; and when the course of the disease is untoward, a period of two months scarcely seems to exhaust its power of producing even acute symptoms.' (*On Rheumatism*, p. 25.) M. Chomel's experience led him to believe that four weeks is about the average period for arriving at convalescence, and *that recovery never takes place before the twentieth day* (*La Lancette Française*, Août 1834), and Dr. MacLeod imagined that with the common methods of treatment, probably five or six weeks may be about its duration. (*On Rheumatism*, p. 25.) My own observation," continues Dr. Fuller, "has led me to believe that, even when unattended by any internal affection, the disease, under ordinary methods of treatment, endures very generally from four to five weeks, and, under unfavourable circumstances, may be indefinitely protracted. Of the 246 cases admitted into St. George's Hospital, under the care of the physicians during the time I held the office of Medical Registrar, the great majority were decidedly convalescent about the end of the fourth or beginning of the fifth week from the commencement of the attack, and were ready to leave the hospital about the end of the sixth week."

In confirmation of my opinion of the superiority of the homœopathic over the ordinary treatment of acute rheumatism, let me refer you to a paper by Dr. Vaughan Hughes, published in the *British Journal of Homœopathy* in 1869. There you will find a table of 31 unselected cases treated in the London Homœopathic Hospital. The heart was affected in 11 cases, the average duration in hospital was 19½ days. Compare this with 35 days' dura-

* *Rheumatism and Gout*, 3rd edition, p. 70.

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tion in St. George's Hospital, an institution which, owing to Dr. Fuller, has gained a notoriety in the treatment of acute rheumatism.

I am also glad to quote the confirmatory opinion of Dr. Richard Hughes, to whom we are so much indebted for his admirable *Pharmacodynamics*. At one time he thought highly of the alkaline treatment, but he now writes in his *Manual of Therapeutics* (p. 63):—

"I think you will have reason to be well satisfied with this treatment; and will not hanker after the flesh-pots of Egypt in the shape of the alkalies with which you have been hitherto accustomed to saturate your rheumatic patients.

"It is a more difficult question whether our specific treatment secures for the *heart* the same measure of immunity which is claimed for the alkaline method.

"In a paper '*On the Chemical Treatment of Disease*,' which I read before the British Homœopathic Society in 1865, I argued the negative of the proposition (you will find the paper in the 4th vol of the '*Annals*'). So many of my esteemed colleagues, however, at the time and afterwards expressed their dissent from my conclusions, that in deference to their judgment I have refrained from carrying them into practice. You will read the discussion on my essay, Dr. Black's paper, '*Am I a Homœopathist or a Physician?*' with the discussion following it; and the review of my paper in the *Monthly Homœopathic Review* of December 1865, and then make up your mind for yourself."

I trust, gentlemen, your answer will be mine, that homœopathic treatment affords as great immunity to heart affection as any other treatment. There is little doubt that though depressing treatment predisposes to cardiac complications, no drug treatment has any prophylactic effect. There is no evidence that the fullest alkaline treatment has any advantage in this respect over the simplest expectant treatment. Rest and the warmth of bed represent all that art can do in this way. Nor is this a matter of surprise; for the occurrence of cardiac affections is no measure of the severity of the disease; it is as likely to be met with in a mild as in a severe case, and it is much more often met with in the first week than in the more advanced stage of the disease—a circumstance which must puzzle those who believe in the acid hypothesis of acute rheumatism. Much depends on the age and previous condition of the patient. Where from any cause the heart is enfeebled, there the predisposition to cardiac mischief is present.

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I hope another Editor of our quarterly journal may make an avowal similar to Dr. R. Hughes, for he has expressed opinions in 1866 in that journal (*British Journal of Homœopathy*, vol. xxiii.), which I think are damaging not only to homœopathy but to accurate therapeutic results. After noticing the results given by Dr. T. K. Chambers, where lying between blankets formed a marked feature in the treatment, reducing, he considers, from 16 to 4 the risk of cardiac complication, he goes on to say:—

"The simple expedient of making the patient lie between the blankets in place of the sheets is not too insignificant to be despised by us. We have repeatedly seen the good effects of giving Bicarbonate of Potash in rheumatic fever, and now never hesitate to administer it in alternation with the indicated homœopathic remedy. Rheumatic fever seems to be just one of those diseases where it is advantageous to practice a little chemistry in addition to the specific treatment."*

My objections to this advice are, that, to practise chemistry, it is wise to have the necessary condition, *e.g.*, a chemist, before he adds an alkali to neutralise, makes sure that an acid is present in his mixture; but where is the acid in rheumatism found?

2nd. Such advice is damaging to all therapeutic results; for what conclusions can be drawn from cases where the system is saturated with bicarbonate of potass, and small doses of specific medicines are given?

3rd. As I shall afterwards show, the supposed good effects of alkaline treatment are denied by various eminent authorities.

I would now draw your attention to a very important question: Are the results I have referred to, whether treated homœopathically or in the ordinary way, to be regarded as cures or recoveries?

A late discussion in the London Medico-Chirurgical Society affords some materials for the answering of this important question.

Dr. Gull, of Guy's, and Dr. Sutton, of the London Hospital, related the history of twenty-five cases which they treated with mint-water, and allowed to run their course naturally.

* See Dr. Dudgeon's explanation in *The Monthly Homœopathic Review*, vol. xiv. p. 697, where he states he has entirely given up the use of alkalies, being convinced of their inutility.

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They say our cases appear to teach us that the rheumatic process runs its course under the expectant treatment as favourably as under the influence of drugs.

In answer to the question—Does drug-treatment prevent the heart from becoming diseased?—they say, of the twenty-five cases, twelve had organic disease on admission; no organic disease supervened during the treatment; the heart was healthy in eleven, and when they left it remained so. They found that when the patient was admitted free from heart disease, this complication rarely occurred during the natural course of the disease and its treatment by the do-nothing system in hospital. They consider that there is not sufficient evidence to prove that any of the advocated systems have power to prevent the heart from becoming diseased.

They, along with Dr. Barclay, agree in the opinion that alkalies have no particular influence in curtailing the duration of the disease.

Dr. T. K. Chambers treated eleven cases without any medicines except an occasional dose of opium. The average duration was thirty days.

As far, then, as expectant treatment expresses the natural duration of acute rheumatism, it may be stated to be from twenty-six to thirty days.

The expectant method is equal to the best results of ordinary treatment, and is very superior to depletory remedies, or to opium and mercury; but how does it stand in comparison with homœopathic treatment?

The return by Drs. Wurmb and Caspar of cases treated uniformly by high dilutions are, as they state, the same as the expectant. The table of thirty-one cases treated by low dilutions in the London Homœopathic Hospital yield much better results, giving 19½ days' duration as compared with 26 to 30 under the expectant. This duration corresponds with an average of 20 days, roughly taken, of cases treated by myself with low dilutions.*

* Since this paper was written, Dr. Hamilton Mackechnie has reported, in the *British Journal of Homœopathy*, vol. xxviii., six cases of acute rheumatism treated in the London Homœopathic Hospital. Case 1. The acute attack supervened on chronic rheumatism, and had existed 14 days before she came under treatment. Duration in hospital, 37 days.—Case 2. Treated from first day of attack. Duration, 16 days.—Case 4. Ditto.—Case 3. Long-standing acute attack, supervening on chronic rheumatism, in an epileptic subject with cardiac disease. Stay in hospital, 16 days. He was dis-

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I think, then, as far as our data guide us, we may consider that *the homœopathic treatment reduces the natural duration of acute rheumatism by seven days.*

From time to time one meets with decided cases of rheumatic fever, where, under the administration of homœopathic remedies, the disease does not last more than eight days, but these are not average cases.

I have no hesitation in coming to the conclusion that homœopathic treatment is superior to the saline plan, and I think future experience will probably confirm my belief that it has a decided effect in modifying the natural course of acute rheumatism.

Let me recommend in the treatment of this disease the administration of full doses of the lowest dilutions. *Let each medicine be given steadily for some time, and not changed on account of trifling indications.* Let the alternating of remedies be as much as possible avoided.

Alternating and frequent change of medicines are very frequent causes of failure in this and many other diseases, and this practice gives a curious medley where accurate therapeutic results are expected.

The remedies I have most confidence in are *aconite, bryonia, colchicum, pulsatilla,* and *rhus.* I think *actea* is worthy of trial. For cardiac complications I employ *aconite, arsenic, bryonia, colchicum, digitalis,* and *spigelia.*

Then there are valuable aids in careful regimen—a diet at first spare and farinaceous, a very gradual return to animal food, and of this kind milk and whey are at first most suitable. Rest in bed between blankets, wearing flannel shirts, an equal temperature, and well-ventilated room, are useful assistants to recovery. Add to these local applications, and none are simpler or better than simple water, or water and spirit compresses to the affected part.

Treated somewhat in this manner, I hope your experience will confirm mine, and add to the proofs that acute rheumatism is considerably modified by homœopathic

missed free of all rheumatic affections.—Case 5. Ill a week before entering hospital. Duration in hospital, 30 days.—Case 6. Subject to chronic rheumatism. This is the third acute attack, and has lasted three weeks before admission. Stay in hospital, 61 days. This lengthened stay in hospital was due to an attack of sciatica which followed the acute rheumatism.

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treatment. The enquiry is one which merits your careful attention, for many a life is rendered miserable by cardiac disease originating in acute rheumatism.

It is an investigation into which we must enter free of prejudice, and with no partisan spirit. Let scepticism open to us the gate of enquiry, but let this be for the admission of truth, and not for the harbouring of a *laissez-faire* spirit, which rests satisfied with doing nothing because nature does so much. Homœopathy has gained a great negative victory in the rapidly-increasing spread of expectant treatment; but it is for us, who are her votaries, to press far beyond this point, and to add to her positive triumphs by the discovery of specific medicines.

Clifton, September, 1870.

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PREFACE.

THE Annual Congress of British members of the medical profession practising homœopathy was held this year at Oxford, on the 27th of September. The following papers were read, and, with the exception of the President's address, fully discussed on that occasion.

About forty medical men were present, and the proceedings were both interesting and practical.

The absence of the President, through very serious illness, was a source of the deepest regret to all. His address, which was read in a clear and impressive manner by Dr. RICHARD HUGHES, was regarded by all who heard it as an essay of a very high order of ability, and as one better calculated to present the claims of homœopathy to physicians whose studies are chiefly limited to questions of pure science, than any hitherto published.

The paper on "The Dose," by Dr. BLACK, proposing as it does a method of experiment, will, it is hoped, aid in reducing the difficulties which have hitherto surrounded the investigation of the questions involved in its consideration.

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The essays by Drs. MOORE, DUNN and WYNNE THOMAS contain a considerable amount of practical information. The discussions each provoked were quite sufficient to prove the great advantage to be derived from meetings such as the one at which they were read.

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} *Hon. Secretaries.*

November 1870.

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ON THE RELATION OF THERAPEUTICS TO MODERN PHYSIOLOGY.

By Dr. Henry R. Madden.

Gentlemen,—Allow me to welcome you to this ancient and renowned seat of learning, and to express a hope that our sayings and doings may be found worthy of the place we have selected for our meeting.

The subject I have chosen for my address is one of such vital importance that it cannot too seriously engage our attention, while at the same time it is so vast that I feel it well nigh impossible to do it justice within reasonable limits. I must therefore crave your indulgence should I, in my endeavour to keep within these boundaries, appear in any degree to have sacrificed clearness of expression to condensation of statement, or should any of the facts referred to be unaccompanied by those proofs of their accuracy which might have been adduced.

As my subject specially deals with the fundamentals of a scientific therapeia, I have to treat first of the fundamental laws of physiology ere I can relate the one to the other.

The visible universe may be said to consist of *matter* and *motion*, both equally indestructible and unproducible. The absolute amount of both is ever the same, and all the variety which we see around us is the result of modifications of the one or the other.

Physical science has taught us that all the forces in nature are modes of motion, and has more than hinted that all the forms of matter may be equally dependent upon the same great agent, while life is so distinctly a *mode of motion*, that on an examination of its laws rests the very foundation of physiology.

In studying motion we must first distinguish between

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the visible movement of masses and the invisible motions of molecules. With the former, however, we need not at present occupy our time.

Molecular movements are of two classes, viz., movements among themselves and movements within themselves.

With *atoms* we can have no practical acquaintance, the smallest mass detectible by the highest microscopic power is still a molecule, or more probably a considerable group of molecules.

Chemistry teaches us that even the so-called elements exist in molecular groups of atoms, and act only when thus associated—nay, there are not wanting those who believe with the late Dr. SAMUEL BROWN, that all the difference between one element and another consists in different groupings of the ultimate particles, so that atoms themselves are groups, and we know absolutely nothing of the one indestructible substance which underlies all phenomena, our utmost knowledge extending only to the products of its groupings.

According to this view, the atomic weight of an element expresses either the relative complexity of its molecule, or the velocity of the revolution of its constituent particles.

Apart from this, however, which is not as yet the accepted theory, although much may be said in its favour, the well-studied subjects of electricity, galvanism, and magnetism, lead almost inevitably to the conclusion that motion exists in all molecules at all times, and that to its modifications are to be traced all the various phenomena to which these sciences relate.

Heat is another mode of motion, and the perceptible temperature of any body is the measure of the rate at which its molecules are vibrating at the time; while radiant heat consists of the same vibrations transferred to the subtle ether that fills all space and envelopes the molecules of the densest materials.

Radiant heat and light are products of the same waves of ether, and differ only in size and velocity; the larger and more slow moving waves causing the phenomena of heat, while the smaller and more rapid vibrations manifest their presence as light.

We thus perceive that the whole field of physics is occupied with investigations relating to modes of motion. It however limits itself to such motions as occur in masses

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and among molecules, and leaves to other sciences the examination of those movements that are more deeply seated.

At this point chemistry takes the initiative, and explains to us that various molecules have the power of uniting together, and forming compounds possessing properties totally distinct from those of either of its constituents. In other words, certain molecules on coming together break up their internal arrangement and combine to form an entirely new arrangement having a new motion, and hence manifesting new properties. From whence it follows that in all chemical compounds the complexity of the internal arrangement of each molecule, and as a consequence its mode of motion, will bear a direct relation to the number of atoms of which it consists—ternary compounds being more complex than binary, and so on. This increasing complexity, however, depends on the number of atoms quite as much as on the number of elements existing in a compound, and hence it is that two elements uniting in the proportion of four atoms with five will produce a much more complex arrangement than if they united as one to one or one to two, or even as one to four.

We have seen that elements are only known in the form of molecules, and that even in this uncombined state the grouping of the ultimate atoms may differ is evidenced by the phenomena of *allotropism.*

We know that the properties of a body are dependent upon the arrangement and mode of motion of its constituent particles, and hence it follows that the atoms of such substances as oxygen, phosphorus, sulphur, carbon, &c., must be capable of assuming and retaining very different arrangements ere they could manifest the very divergent properties characteristic of their allotropic states. While on the other hand the facts of *Isomerism* clearly demonstrate the same capability to exist in compounds. The almost innumerable and utterly diverse forms in which Carbon and Hydrogen when united in the proportion of ten to sixteen can appear, is shown by the fact that no less than twenty-three volatile oils, including such very different substances as *turpentine, chamomile, clove, juniper,* and *valerian,* possess the same proportion of these two elements.

Another great advance in complexity of arrangement is

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found in what Graham has named the *colloid* condition. The late Dr. Miller writes: "The chemistry of a body in the colloid condition is very different from that of the same body in its crystallised form." And again, "The combining proportion of colloid is generally high, although the ratio between the elements of the substance may be simple, and it seems not to be improbable that the grouping together of a number of crystalloid molecules may be one of the essential requisites for the development of the colloid condition."*

If then such a compound as C_{10}, H_{16}, is capable of so many modifications, what might we not expect from a colloid-like albumen, whose formula, according to Dr. Miller, is C_{72}, H_{110}, N_{18}, SO_{22}, H_2O, or in other words 226 atoms of five elements at least, two of which we have already seen to be remarkable for their allotropic modifications?

Most colloid substances belong to the organic kingdom, and it is well known that no crystalloid can become part of a living organism, until it has been brought into the more complex colloid state.

During life, however, the constitution of the molecule is still more complicated than in the colloid condition, and the act of death evidently consists in the breaking up of this most complex of all molecular arrangements into the less complex colloid, and the more simple crystalloid forms of matter. This condition has been termed by Dr. Drysdale *the metabolic state*, and is especially characterised by the following peculiarity, viz., that all ordinary chemical affinities are suspended. This is supposed to be occasioned by the circumstance that its constitution is perpetually changing, so that ordinary chemical compounds cannot be formed. Most chemical combinations require time, and the rate at which the constituents are brought together and the duration of their contact will materially influence the result; so much so indeed that advantage is taken of this fact in the arts to accomplish purposes which would be otherwise unattainable. For example, before calico can be printed every loose particle of cotton must be removed from the surface, in order that the coloured inks may not run and damage the clear outline of the pattern. This removal is effected by passing the calico over and in contact with a red hot iron cylinder,

* *Miller's Chemistry*, 3rd Edit., Vol. III, p. 88.

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and by regulating the rapidity with which the cylinder revolves and the calico passes over it, the intense heat burns off the loose fibres and yet does no injury to the woven cloth. In other words the changes in the relation of the high temperature and the cotton are too rapid to admit of the fibre combining with the oxygen. Let the rate of revolution be reduced but a very little and the calico would burst into flames. Again it has been found that certain fulminates can be detonated in contact with gun-cotton without causing the latter to explode; and experts account for this on the ground of the extreme rapidity with which these fulminates expand, too rapid indeed to enable the *pyroxyline* to initiate its new mode of motion, and hence it remains unchanged. Precisely the same kind of thing occurs in the metabolic state of matter. It can only last so long as rapid and incessant changes are going on, for which purpose it must always be in contact with living matter; and if the rate of these changes is reduced beyond a certain point, the chemical affinities of the materials will at once assert themselves, and the whole will break down into more or less stable chemical combinations.

My subject is far too vast to admit of my bringing forward proofs of all these assertions, but they will readily be found by all who are conversant with recent writings on these subjects.

We have now, therefore, reached the confines of physiology, and we have seen four classes of complexity, each of which contains many degrees, viz.:—

1st. *Atomic complexity*, varying in accordance with the atomic weight of the element.

2nd. *Chemical complexity*, varying according to the number of elements and the number of atoms of each which go to form each chemical combination.

3rd. *Colloid complexity*.

4th. *Metabolic complexity*. We have likewise seen that this last and most complex molecular arrangement of all must be reached ere the phenomena of life can be manifested.

Approaching the subject from a different side we may state, that the simpler the molecular arrangement, the more stable is the mass, and the fewer are its properties; while conversely, the more complicated the molecular

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arrangement, the more readily can it be interfered with, and the more numerous and diverse are its properties.

We are therefore prepared to find the simplest living organism possessed of many and various properties, and capable of being readily altered by a variety of causes.

The invaluable researches of Dr. Lionel Beale have proved that the only *truly living* matter is a "pulpy, translucent, homogeneous matter, yielding, after death, fibrin." This is the *germinal matter* of Beale, the *protoplasm* of many recent writers. All visible structure comes from this, but according to Beale, *all formed matter is dead;* an assertion which in the highest sense is perfectly true.

In our physiological researches, therefore, we have here a fresh starting point. This structureless pulp, this *germinal matter*, is as we have seen the maker of all structures.

Every living cell, whether animal or vegetable, consists of one or more masses of germinal matter enclosed in the cell-wall. This cell-wall is just as much produced by the germinal matter as the cocoon is produced by the silk-worm. As the larva consumes food (pabulum) and converts it into its own substance, and from thence produces the silk and spins the cocoon, so germinal matter receives pabulum, converts it into its own substance, and from thence produces the albumen, or fibrin, or whatever else constitutes the material of the cell-wall; and therewith builds the wall around itself. Unless therefore it would be proper to call silk living so long as the enclosed larva is alive, it cannot be correct to call the cell-wall living simply because it contains live germinal matter.

From the simple cell constituting a *monad*, to the most intricate structure constituting the cerebral hemisphere in man, we have precisely the same series of phenomena: everywhere there is germinal matter, and everywhere there is formed material. Among much that is deeply interesting in this field of research, the one essential thing for us to notice is, that as we ascend the scale we find greater and greater complication of arrangement, and hence an ever-increasing capability of becoming deranged.

The extremely complex molecular constitution of germinal matter, or in other words, of molecules in the metabolic state, is essential to its possessing that peculiar

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property to which Fletcher gave the name of "*irritability*," or the power to respond to stimuli; or, to express it in other words, the property of changing its condition in response to impressions. That a molecular arrangement far more simple than that of germinal matter is sufficient to render it responsive to certain impressions is well known. For example, *biniodide of mercury* can be obtained in large lemon-yellow crystals, which retain their colour if perfectly undisturbed; a very slight shock, however—for example, a mere scratch with a needle—will produce a vermilion colour at the point of contact, which will rapidly spread all over the crystal, however large its size, and indeed very frequently over the entire mass of crystals lying in contact with each other. Now the laws of optics teach us that the physical cause of colour lies in the molecular arrangement of the coloured mass; and hence in this case a change of molecular arrangement must have been set going by the very small mechanical disturbance of one point of the surface of our crystal of *biniodide of mercury*. The firing of a charge from a needle-gun is another well-known instance of the stimulus of pressure on one minute spot, originating a sudden and entirely new molecular arrangement of the whole mass.

Fletcher long ago pointed out, and Drysdale has recently reminded us, that three things are essential to the existence of life, viz.:—

Irritable matter, to act.
Pabulum, to be acted on.
Stimulus, to cause action.

In all animals and vegetables, from the highest to the lowest, these constitute all that is essential to life; and the *changes which irritable or germinal matter undergoes, constitute the whole of the phenomena of life.*

The incalculable variety of structure and action manifested by living beings is but the record of the variety of properties which characterise "pulpy, translucent, homogeneous matter, yielding after death fibrin."

Dr. Beale says that germinal matter is always the same, no matter from what source it is obtained; but Dr. Drysdale well remarks that this applies only to its microscopical and chemical character, and not to its most important and characteristic property, viz., the way in which it responds to stimuli; in this respect there are as many species of

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germinal matter as there are diversities of structure and function throughout the organic world.

We must now return for a while to consider the various modes of motion.

The doctrine of the correlation of forces has shown the indestructibility of motion, and in consequence its ceaselessness. Whenever motion seems to disappear, it is owing to its having been transferred from masses to molecules. When motion becomes heat, as in the boring of a cannon, the locomotion of the mass has been shivered into the thermal vibrations of the molecules. When light is absorbed the rapid waves of the ether have been massed into the slower swing of the molecules of the absorbing bodies, and so on through all changes; what seems to be *coming to rest* is simply a change of the mode of motion, and what is termed the *storing up of force*, is in like manner the accumulation of molecular motion within the receptive mass.

A very simple and instructive example of the storing up of force is seen in the American machine termed "*The Accumulator*," by means of which the lonely bushman can at any time obtain the power of ten or twelve men. It consists of a series of indiarubber cords, each furnished with a hook at one end and a ring at the other, and when the person wishes to move a weight far heavier than his strength can accomplish, he fastens a strong rope round the object to be moved, and drives a stake firmly into the ground at such a distance from it that the india-rubber cords must be fully stretched before they can reach from the one to the other. He then takes one cord of the caoutchouc, hooks it into the rope which surrounds the mass to be moved, and then stretches it until he can pass the ring over the stake. To do this he must exert his full strength, and as the caoutchouc remains stretched an equivalent traction-power to his full strength continues to drag on the mass. Proceeding in like manner to apply cord after cord, he obtains a traction equal to as many men as he has stretched cords; and at last, on stretching another cord, the mass is moved. Here we have manifest storing up of force in the stretched cords; and yet there is no apparent motion, all visible motion has ceased and been replaced by what is usually termed "tension" or "potential energy." I have purposely, however, avoided this word in order to show that what is called tension is also a mode of motion.

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Let us examine the condition of the caoutchouc before and after it was stretched. In its passive condition the molecules of caoutchouc are evidently swinging freely through a comparatively large space, so large as to admit of their readily moving upon each other in all directions; and it is this condition that renders the substance capable of being stretched at all. At the same time the attraction between its particles is very strong; this attraction being also a mode of motion, as indeed are all the manifested properties of matter. When, however, it is fully stretched, the force exerted in stretching it has altered the arrangement of its molecules so materially that it is now hard and brittle, instead of being soft and elastic, so brittle indeed that any extra force would break it; hence the wide swing of the soft elastic mass has been broken up into the short rapid molecular movements of a hard, brittle substance, and this change represents an increase of molecular motion exactly equal to that expended by the man in stretching the cord. Thus we perceive that "tension" or the "storing up of force" really means the transfer of motion from masses to molecules. Why caoutchouc has the power to resume its original mode of molecular motion as soon as it ceases to be retained in its stretched condition, while an equally extensible mass of glue would remain permanently in the stretched condition, is for the present inexplicable. It constitutes a *property* of the caoutchouc, and in this manner serves to differentiate it from substances which are ductile and plastic, but inelastic.

There can be little doubt that variation in molecular arrangement lies at the root of all the properties of all differing substances, but it is essential to remember that *motion imparted to a substance and not passed on or changed into some other visible form of motion, is equalised by some new form of molecular motion in the substance thus retaining it.* Those who wish to enter more fully into this question should study carefully Dr. Drysdale's second chapter on the *Nature and Definition of Force*, in the twenty-eighth volume of *The British Journal of Homœopathy*.

The next point we must bear in mind as necessary to a correct knowledge of our subject, is the essential distinctions between *properties* and *force*. I will therefore endeavour to formulate these before proceeding farther.

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Force is distinct from, though only conceivable as acting upon, matter.

Properties are attributes of matter.

It is quite possible to conceive of matter void of force, but it cannot be conceived as void of properties.

Force is—and must be always—in exercise.

Properties may be in exercise or not.

Force is *motion*, one and indestructible, it may vary in mode of manifestation, but cannot vary in amount.

Properties are infinitely various, may be destroyed and reproduced; are absolutely dependent on circumstances, changing or unchanging as the circumstances vary on which they depend.

Throughout the universe matter does not exist without force. All the forms of motion, all the force in this world is derived from the sun; and a fresh supply is constantly required to compensate for the loss which this world sustains in the form of radiant heat and light incessantly passing out beyond the limits of our atmosphere.

We may sum up this part then with the assertion, that *force, acting upon matter, calls its properties into exercise, and the result is in every instance regulated by the nature of these properties.*

One body may act upon another in two very different ways. 1. It may simply transmit its own motion, as when one ball is pushed against another, and passes on to it a portion of the force with which it was impelled towards it.

2. It may set free an amount of motion far greater than what it transmitted, in consequence of the matter acted upon having already a store of force ready to be called into activity, as when a cap with a hammer explodes a mass of *dynamite*, or any other detonating mixture. Now this latter mode of action is termed *stimulation*, and the acting body is termed a *stimulus;* and we must never forget that a mechanical agent produces an effect which is the exact equivalent of the force it transmits, while a *stimulus* produces an effect greater than the amount of force it transmits—the increase being always set free from a store previously existing in the matter acted on. There is thus an apparent, but no real production of force. The excess is extricated, set free, not produced; and the substance acted upon invariably suffers loss. It loses more or less of some property which it possessed in virtue of the store of force laid up in it. For instance in the ex-

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ample quoted, *dynamite* possessed the property of sudden and enormous expansion; but as soon as its store of force was liberated this property was lost; and seeing that this very property was an essential characteristic of the *dynamite*, this latter at the same moment ceased to exist, and its elements arranged themselves in entirely new forms.

Let us therefore bear in mind these three things, that: 1. *The effect of a stimulus is greater than its mechanical power.* 2. *Matter which is capable of responding to a stimulus must possess a store of force.* 3. *When any matter responds to a stimulus it invariably suffers loss.*

I need hardly remind you that that which Dr. Fletcher calls *irritable matter*, and which Dr. Lionel Beale calls *germinal matter*, is capable of responding in a marked degree to numerous stimuli; and we have seen that this is owing to the extremely complex character of its molecular structure: for, be it remembered, every complication of molecular structure involves an increased amount of molecular motion, and hence each step in the complexity implies a storing up of force.

We have thus again reached the confines of physiology from the side of force, as we had previously from the side of matter; and we are now prepared to pass rapidly *in medias res*.

The ascent from the monad to man is a constant series of complications or unfoldings, by which force is stored and properties are multiplied. The complications, however, are of two kinds, viz., complications of structure and complications of molecular arrangement. The mass becomes more and more differential, and the molecules composing it become more and more complex in their properties.

Every organ of the body consists essentially of cells containing germinal matter; and every difference of function depends upon the equivalent differences in the properties of this "homogeneous translucent mass," these again are the evidence of a different molecular constitution.

Every cell, therefore, possesses an independence traceable to its peculiar constitution; and every cell acts according to this peculiarity, and cannot act in any other way until its molecular arrangement is changed.

Germinal matter—being irritable—responds to stimuli, and in so doing suffers loss, hence, while it lives (and it

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only retains its peculiar constitution during life), it must receive a constant renewal from without, and this renewal must, in its turn, possess a store of force. We must, therefore, have *pabulum*, then we have our three prime requisites of life, viz., *germinal matter*, *stimulus*, and *pabulum*.

Each mass of germinal matter increases by the absorption of pabulum, and co-incidentally produces some formed matter, either a structure or a formed secretion.

All *formed material*, whether structural or amorphous, is no longer in the metabolic state, it is reduced to the lower grades of colloidal and chemical complexity; and hence force is set free, and either passes on as one of the known physical forces, or is employed in raising the absorbed pabulum to a higher degree of metabolic complexity.

The time during which an individual molecule retains its identical metabolic state is probably very short indeed, there is an incessant passing upwards and downwards of molecules from one degree of complexity to another, so that the condition is essentially one of change so long as life continues; and the essential point for us to remember is, that every step of the upward progress increases the amount of stored-up force (molecular motion—tension), while every descending step sets force free.

Individual masses of germinal matter are possessed of three most important properties, viz.:—

1. The power to undergo molecular changes on the application of a stimulus.

2. The power to *reproduce its like*, or, translating this into a mode of motion, the power to impart its own molecular motion to molecules of an analogous kind, thus prolonging changes of motion.

3. The power of returning after a time to its original mode of motion, and thus terminating changes of motion.

This is claiming for the individual molecules what Darwin has so abundantly proved as regards the entire living creature, viz., the power to perpetuate a change, and the tendency to revert to original forms.

Practically one cannot exaggerate the importance of these facts, because if the molecule had not the power to perpetuate change, no disease could have any duration—the molecule must either retain its health or die. If again the tendency to revert was lost, no disease could be cured—a change once produced would be perpetuated.

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With these facts before us respecting the constitution and manner of life of each separate mass of molecular matter, we may proceed to the examination of the entire animal.

The body, occupying a large space, consisting of nearly twenty-three millions of cells, and performing all its complex functions, is an undivided whole—an *individualism*. The independent cell-life must, in some manner, be associated together; and this is effected in the most perfect way by a two-fold circulation, which continues unceasingly throughout the entire mass, viz., the circulation of pabulum (the blood), and the circulation of nervous influence. The first associating all the cells together as a matter of life and death, being all dependent on one commissariat. The second associating them for unity of action, being all under one government.

Let us very briefly consider these two circulations upon which the unity and integrity of the body absolutely depend.

1. *The circulation of the blood.* Recent observation has added largely to our knowledge of blood-making. The most important point of which, for our present purpose, is that every particle of food must have again and again become part of living and growing tissue ere it enters the blood-stream. Just as no animal can live directly on inorganic matter, but must receive its elements from the vegetable kingdom, so in the higher animals all food must have become the germinal matter of intestinal cells, of colourless and other corpuscles, and of various lacteal or lymphatic glands, ere it is fit to be mingled with the blood. And again, after entering the circulation, the same ascending series of changes takes place; in fact there is the highest probability that in every change of germinal matter, except that which occurs in the ideational centres, three things occur: a certain number of molecules, pre-existing in the pabulum, are raised to the level of the germinal mass that has absorbed them, a small number of those pre-existing in the germinal matter attain a higher degree of complexity of arrangement, and pass on to be the food of higher tissues; and a larger number, probably derived partly from germinal matter, and partly from pabulum, descend to the level of formed material. The change consists entirely of a re-distribution of molecular motion; there is no additional force gene-

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rated, but what is set free by the degradation of some molecules is employed for the elevation of others.

Blood must not be viewed as a chemical mixture, for such it does not become until death. In the living body it consists of water containing matter in the crystalloid, colloid, or metabolic state, the last of these being capable of yielding molecules of vastly different degrees of complexity of motion; and just as the sun sends forth heterogenous rays which can be separated into different parts by sorting together the waves of equal length; and just as different natural substances have the power of absorbing certain of these, and letting the others pass by, so the blood contains molecules of matter in the metabolic state; and each part of the body has the power of differentiating from it portions capable of assuming a mode of motion similar to that pursued by its own molecules.

That this apparent power of selection is a purely physical act is rendered highly probable from analogous changes which are perpetually occurring around us. These have been beautifully illustrated by Tyndall in his *Lectures on Sound*, where, speaking of sensitive or singing flames, he points out that each flame has its own particular note to which it responds; and that, although its sensitiveness to that particular note may be extreme, any number of other notes may be sounded loudly and continuously without producing the slightest influence upon it. This certainly looks quite as much like selection as anything which occurs in the living body, yet it is well-known to be a purely physical effect.

When the *liquor sanguinis*, containing the pabulum of all the tissues, passes by any cell, a portion enters by endosmosis, and comes in contact with the living germinal matter therein contained; being sensitive to all kinds of molecular movements, it responds to that of the germinal matter; and at once splits up in different rates of motion, part assuming the same mode of motion as the germinal matter itself, and thus attaining similar properties and becoming identified with it, while the remainder falls down to a lower rate of motion, and, with the other fluid contents, passes out of the cell by the exosmotic force which impels it forward.

While the constitution of the blood is thus for ever changing, its general composition remains unaltered so long as health continues: a marvellous result arrived at

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by the inter-dependence of all the various tissues on each other; and by the varying rates of nutrition of different parts of the body; a result, however, necessitating so nice a balance that it is easily disturbed, and thus constitutes a most fertile source of disease.

The pabulum being conveyed to all parts by the blood, the amount supplied is, of course, regulated and controlled by the circulation; and this again depends on the calibre of the arteries. Here, therefore, we touch the second circulation, viz., the nervous, for on it depends the contraction and relaxation of the blood-vessels. It would be obviously out of place for me to enter fully into the physiology of the nervous system, as my hearers must all be familiar with the subject. It is essential, however, that I should specially direct your attention to a few points.

The nervous system, as at present known, is one of the most perfect systems of representative government that can be conceived.

The distal terminations of afferent nerves are the first recipients of impressions; the nerves are conductors; and there are four sets of nervous centres to which the impressions are conveyed.

Beginning at the lowest, Dr. Maudsley, from whose admirable work on *The Physiology and Pathology of Mind*, I have gathered much of my knowledge on this subject, remarks: "The ganglionic cell of the sympathetic co-ordinates the energy of the separate elements of the tissue in which it is placed, and thus represents the simplest form of the principle of individuation." In other words the separate cells of each tissue are linked together by the minute ganglia of the sympathetic, and are thus enabled to work together. These united cells next communicate with the ganglionic matter of the spinal cord; and if the message sent relates to a matter of which the cord has full cognizance and control, the answer is sent back direct, and the higher centres are not troubled with it,—this constitutes *reflex action*. If, however, the cord is disturbed by the message, it passes it up to the *sensory ganglia* at the base of the brain, and from thence receives a message how to act,—here we have *sensation*. It often happens, however, that these centres are also sufficiently disturbed to be induced to send the message still higher, and then it reaches the Throne itself, the *ideational ganglia*

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of the cerebral hemispheres,—and then we have *consciousness*. Dr. Maudsley well remarks that "each centre is *subordinated* to the centre immediately above; but is capable of determining and maintaining certain movements of its own without the intervention of its supreme centre." The strictest regularity exists in all these actions —each centre does its own work, when it has once learned it, thoroughly; but always refers to the higher centre in cases of doubt. Again, each higher centre in conducting its operations, makes use of the centres below as the proper and accredited channels of communication.

One of the most valuable subjects which Dr. Maudsley insists upon, and has developed most admirably and convincingly, is the "education of nervous centres," which is constantly going on during healthy life. According to this view, every display of energy leaves its mark behind it, which, becoming stronger and more distinct by every repetition, becomes at length the settled habit of the part. Dr. Maudsley expresses it as follows: "With the display of energy there is the co-incident change or waste of nerve-element; and although a subsequent regeneration or restoration of static equilibrium takes place by the quiet process of nutrition, yet the nutritive repair, filling up the loss which has been made, must plainly take the form made by the energy and co-incident material change. Thereby the definite activity is to some extent realised and embodied in the structure of the spinal cord, existing there for the future as a motor residuum, a *potential* or *abstract* movement: accordingly there is thenceforth a tendency to the recurrence of the particular activity, a tendency which becomes stronger with every repetition of it . . . . . the faculties of the spinal cord are thus gradually formed and matured."* The formation of these organized residua, or rather organized groupings of cells, explains a vast number of phenomena both of health and disease. It explains all habits of body and mind. It accounts for the difficulty experienced in performing novel complex motions compared with the automatic ease, amounting even to unconsciousness, with which the same are effected after long practice. It throws light on the tendency to the recurrence of the series of morbid phenomena in many chronic diseases; and it explains the strange as

* 2nd Edit., page 76.

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sociations between apparently unconnected organs in many nervous patients.

According to Dr. Maudsley, "a spinal cord without memory," (*i.e.*, without organized residua), "would be simply an idiotic cord, incapable of culture." The same authority tells us that the groupings of cells, which exist at birth, consist of those associated with instinctive actions. hence these do not require to be learned. All other groups are organized by culture, and continue to be formed so long as the living being learns anything new.

I need hardly remind you that by far the greater part of the work done by the body is done by muscular fibre, it is well, therefore, for us to examine a little into the manner in which this is accomplished. Among the more recent explanations of the mechanism of muscular contraction, I am most inclined to agree with that advanced by Dr. Lionel Beale. He assures us that he has seen the ultimate nerve fibrils forming loops round the muscular fibrilla; and he views muscular contraction as analogous to the magnetizing of soft iron by a current of galvanism passing through a coil surrounding it. Physical science teaches us that such magnetization depends on an alteration of molecular arrangement occurring in the iron so long as the galvanic current flows through the surrounding coil. The microscope also teaches us that the rods (disdiaclasts) which exist in the cells of muscular fibre assume a different relative position during contraction, and thus cause the shortening of which this contraction consists; and, according to Dr. Beale, this change of position occurs whenever a nervous current is passing: so that muscular contraction represents a passing nerve current, while relaxation represents nervous repose. This is of importance, because it harmonizes with facts of recent observations, such as the non-oxydation of muscle during action; and the nervous exhaustion consequent upon violent muscular efforts. If muscular contraction were indissolubly connected with muscular waste, many phenomena of daily occurrence would be inexplicable, more especially the fact established by Dr. Parkes and others, respecting the elimination of nitrogen; as also the well-known fact that involuntary muscles never tire, at least in a state of health.

Sir James Paget has suggested a most ingenious explanation of this absence of fatigue of organic rhythmical movements such as those of the heart, of respiration, of

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cilia, &c., viz., that they are dependent on "rhythmical nutrition," *i.e.*, "a method of nutrition in which the acting parts are at certain periods raised, with time-regulated progress, to a state of instability of composition, from which they then decline, and in their decline discharge nerve-force."* In other words, the ganglionic cells, forming the central termination of the nerves supplying such muscles, grow and die, setting free their stores of force with a rapidity commensurate with the rate at which the muscles contract, a rate of growth by no means inconceivable, since it is equalled, if not surpassed, by the rate of development and rupture continually going on in the epithelium of secreting surfaces. Add to this regulated rapidity of growth the fact that the entire process is without consciousness, and, hence, makes no demands on the higher nervous centres, and we at once see that, so long as the organism works healthily, fatigue of such parts is impossible. The process goes on with all the steadiness of a water-wheel supplied by an uninterrupted stream, the force required for the work is always ready, each minute's supply being yielded by the nervous system with as much regularity as the running stream sends down its wonted number of gallons of falling water.

To sum up what we have so rapidly sketched concerning the nervous system: We find that the linking together of the body into one is effected by the nervous system in such manner, that what would otherwise be desultory and chaotic assumes regularity and design. We find a series of centres placed one above the other, each having higher and more important duties to perform than the one before; and each consisting of molecules of a higher order of metabolic complexity, and, hence, possessing a larger amount of stored up force. We find each centre doing all the work it has learnt to do without referring to the higher powers; but ever seeking help when in difficulty; and, as a consequence, we find that fatigue is only felt when something new or unaccustomed has to be effected, or when, from disease, the balance between demand and supply has been lost.

While, however, the actions of all the various organs of the body are directed by the nervous system, and their nutrition regulated by the amount of pabulum which they

* Maudsley, Op. cit., p. 81.

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receive; still the action performed in each case is absolutely dependent upon the properties of the cells constituting the organs themselves. Let these properties be altered in any way, and the action will differ, though the nervous stimulus remains the same. We must now, therefore, enquire a little into the nature of stimulus; and, as this is by far the most important point of all in relation to Therapeutics, I must be excused for entering into it somewhat minutely.

Recalling what I have already said with regard to germinal matter in the metabolic or living state, we must realize to ourselves molecules having a highly-complex internal arrangement; and, as a consequence, capable of undergoing an almost infinite number of modifications. Such a molecule left to itself would be incapable of remaining unchanged. The various and antagonizing movements going on among its elementary atoms would inevitably shake it to pieces; and out of the ruin would be produced numerous frail arrangements of a simpler order. In other words it would pass downwards through the colloid and chemical gradations until it rested in the more stable forms of simple, inorganic, chemical compounds. The only possible way in which the metabolic state can continue to exist is by the perpetual addition of fresh matter so constituted as to be capable of taking part in the intricate movements going on; and which are unceasing so long as life continues. In one sense, therefore, this pabulum may be called a stimulus in so far that it renders active the property, possessed by all germinal matter, of reproducing itself.

Under ordinary circumstances, as long as germinal matter receives a supply of suitable pabulum, the presence of the latter determines a definite activity of the former; and a uniform series of changes goes on, consisting, as we have already seen, of the increase of the germinal matter, and the production of formed material. Experience, however, teaches us that any change in the circumstances in which the germinal matter is placed, as also the presence of any unusual matter, may greatly modify or entirely change the character of the result; a change which usually shows itself in the character of the formed material. For example, under conditions of health the germinal matter of the epithelial layer of the mucous membrane produces only epithelial cells; and the surface of the membrane

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exhales water in sufficient quantity to be always moist; a very slight change, however, in its circumstances will cause this germinal matter to appropriate pabulum too rapidly, to form the cell-walls hastily; and, as a consequence, a number of unmoulded cells are set free and floated off in the water with which they are surrounded, thus constituting a mucous flux. Let the change of circumstances be somewhat greater, and the rapidly-formed cells will manifest a new property, viz., that of forming oil globules as well as cell-walls; and then we have pus in place of mucus. As a consequence of still further changes, the pus formed may be no longer the bland, creamy matter which covers a healthy sore, but a thin, acrid ichor which modifies the life of every portion of germinal matter with which it comes in contact. A still further change may render the germinal matter incapable of forming solid cell-walls; and, as a consequence, the mucous membrane becomes denuded of its proper covering, mass after mass of germinal matter is washed away; and we have an ulcerated surface. Now in all these instances the germinal matter has been stimulated in some unwonted manner, and has responded to the stimulus by doing unwonted work. It follows, therefore, that everything which can change the actions of germinal matter or pabulum, is a stimulus; and seeing that in all the above instances the germinal matter and pabulum did all the work, no new matter having been added, the stimulus must have acted by changing the inter-molecular motion of the mass, or, in other words, it set the change going, but took no further part in the result produced. All this accords with what we have here already seen concerning stimuli, viz., that they produce an amount of change far greater than their physical force will explain; and also that the amount and duration of the change has no necessary connection with the quantity or permanent presence of the stimulus. For instance I take a needle, and using each time precisely the same amount of force, I scratch gently the following substances, a piece of glass, a piece of lead, a piece of wax, a cake of fulminate of mercury, a crystal of yellow biniodide of mercury, a healthy man's arm, and a diseased person's arm. The physical force employed in each case is precisely the same, the substance employed is in every instance identical, but the result differs as widely as possible. On the

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glass no visible effect is produced; on the lead a small portion of the surface is scraped off; and on the wax a depression is made, and the displaced material is accumulated like a ridge on either side. In these three cases that needle has acted purely mechanically; and the amount of work done is exactly commensurate with the form and hardness of the needle, and the amount of force exerted in pressing the needle against the substance operated upon. In all the other cases, however, the scratching acted not as a mechanical force, but as a stimulus; and the result was not commensurate with the force applied; but was in each instance dependent upon certain peculiar properties of the substance acted upon. For example, in the cake of fulminate of mercury the stored-up force connected with the possible re-arrangement of its elements is so great that the mass is eminently unstable; and the little scratch disturbed the equilibrium so completely that an entirely new arrangement of its elements instantaneously took place, the molecules rushing with destructive violence into their new position. In the yellow crystal of biniodide of mercury a change of molecular arrangement commenced at the point acted upon, and spread from thence through the whole mass. Now it is evident that the result in these cases cannot be measured by the force employed. No addition to the force would have apparently altered the violence of the explosion of the fulminate; neither was a continuance of the scratching necessary to complete the change in the biniodide. The scratch was the stimulus, but the result was entirely dependent upon the properties of the substance scratched. Precisely in the same way, and for the same reason, the scratch on the two arms produced entirely different results. In the healthy arm a line of roughened epidermis, and a slight capillary injection of the skin, were the only visible results, and in a short time these disappeared; whereas in the diseased arm a series of changes were initiated which, passing through the stages of irritation, inflammation, suppurative and pyæmic fever, terminated only in the death of the patient. In this instance, also, nothing was added to the body, nothing remained in action; but the properties of the parts acted upon being entirely different, the result was equally diverse. Here, also, the dose had little or nothing to do with the result: a single scratch in a sufficiently diseased

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person being just as capable of producing fatal disease as a dozen.

The next point for us to attend to is, that while the changes which take place in germinal matter are directly dependent upon its internal molecular arrangement, nevertheless the agent effecting these changes is invariably a mode of motion, or, in other words, physical force; and there is absolutely no proof whatever of the existence of such a thing as *vital force*, neither is there the slightest need for the conception of such a force. Living matter differs from dead material in its *properties;* and these alone are sufficient to explain all the phenomena; hence we must be careful to speak and think of *vital properties*, and to abjure altogether the false and misleading term of *vital force.*

An illustration taken from physics will aid us much in understanding the complex actions and re-actions of the living organism, will teach us much respecting the nature of disease, and will also point the way to its correct scientific treatment. Let us picture to ourselves a large establishment wherein are made various manufactures in iron, steel, brass, wood, &c., all of which are received in their crude states. We should thus have in one building a number of different machines, each constructed so as to do its own appointed work; and each requiring to be supplied with the material upon which it is to exert its formative power. In the case before us the machines themselves are constructed of the same materials as the substances which they elaborate; and it is easy to conceive that numerous machines, serving very different purposes, might be composed of the same relative proportions of iron, steel, brass and wood; and, moreover, it is equally conceivable that the manufactured products might also contain the same relative proportions. In such a case chemistry would tell us that the manufactory can only turn out in a completed state the same quantity of iron, steel, brass and wood which it receives as material to work up. It could also analyse the machines; and finding them to consist of the same ingredients in the same proportions, it could explain the chemistry of the entire process; but it would have told us absolutely nothing of the manufacture itself; and, moreover, the machinery might get out of order to an extent rendering it quite incapable of making any useful article, and yet the

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chemist might find nothing wrong. Let the physicist next examine our manufactures, and, tracing every process from beginning to end, he will find no process which he cannot explain; every result is the direct consequence of the force employed, and the effect in every instance precisely corresponds in amount to the quantity of force expended; but here, also, so long as he contents himself with measuring the temperature and calculating its mechanical equivalent; so long as he busies himself with elasticity, weight, expansion, contraction, &c., he will throw no useful light upon the intricate working of any one of the machines, nor will he show how any errors are to be corrected. Before this can be done each piece of apparatus must be studied anatomically, as in this way alone can its mode of motion be ascertained. The amount of mechanical force exerted in moving its several parts teaches but little, we must know all the possible motions of each individual part, and their mutual influence; and we must be accurately acquainted with their order of sequence, ere we can judge of the effect of the whole. This, even, is not enough, as most manufactures require their several parts to be prepared before they are fitted together, so as to ensure perfect articles; and since these preliminary operations, though essential for the perfection of the manufacture, are by no means necessary for its mere putting together, they may be slurred over, or omitted, without the defect becoming at once apparent. Lastly, in such a manufactory there must be a large staff of workmen, each attending to his own work, and each obeying orders received from those placed over him. We have now an analogy to the living body sufficiently accurate to clear up many points of difficulty. The workmen of course represent the nervous system, the machines are the germinal matter, the products are the formed material; and the material brought into the manufactory is the pabulum. Glancing at our simile we at once perceive what will constitute perfect health, viz.:—

1. Every man fully attending to his own business, and giving and obeying orders. (Healthy nerves).

2. Every machine in perfect order, and made of the best materials. (Healthy organs).

3. A sufficient supply of the crude articles on which it works. (Healthy food).

4. The thorough preparation of this crude material

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before using it, to make the more important manufactured articles. (Healthy assimilation.)

We can likewise, with equal facility, perceive what will constitute disease:—

1. Any man neglecting his duty. (Disordered nervous system.)

2. Any derangement in any one of the machines. (Disease of tissues.)

3. An insufficient supply of crude material. (Defective food.)

4. Imperfect preparation of crude material. (Mal-assimilation.)

I need hardly remind you that this simile must not be pushed too far, since it is in many respects very imperfect, more especially as regards the inter-dependence of the various parts: for example, the nervous system performs its work by means of the force set free during the degeneration of its ganglionic cells; and hence is as much a machine as any other part of the organism, and, consequently, equally dependent with the rest on a proper supply of assimilated pabulum. Moreover, the source of motion in our manufactory is totally different from that in the living body; and could only be analogously represented by supposing each machine to be moved by the force extricated during the chemical decomposition of certain parts of the machinery; added to which the living body has two peculiarities which are inconceivable by any mechanical arrangement, viz., 1st, That it cannot cease from work without tumbling to pieces, and, 2ndly, That it never makes its products directly from the material received, but forms them from its own substance, and repairs the damage thus induced with the materials supplied to it.

Notwithstanding these imperfections, however, it will serve our purpose in rendering intelligible many important points. Let us follow some of the crude material, say iron, from its reception into our establishment to its exit as formed material. The iron is received as a mass of cast-iron direct from the furnace where it was smelted; and in this state is only fitted for the casting. If needed to make any piece of machinery where toughness is required, it must be forged and rendered malleable; if wanted for other purposes it must be converted into steel; or again it must be hardened, in other words, according

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to the purpose for which it is ultimately required, it has to undergo various processes by which its molecular structure is modified; and unless these modifications are properly effected, the resulting manufacture will be imperfect. If, for instance, a piece of brittle cast-iron is put where tough wrought-iron should have been used, the article will break when exposed to a strain which, if properly made, it would have been fitted to endure; or if tough, soft iron is used where an elastic, springy steel should have been placed, the article will be found wanting in one of the most essential characteristics. Precisely analogous changes go on in the living body; the crude pabulum received from the *primæ viæ* must undergo many changes and modifications of molecular structure ere it is fitted to supply the wants of the higher tissues; and if any mass of germinal matter is obliged to assimilate pabulum lower in the metabolic scale than is best for it, the result will be a change in its own molecular constitution; and, as a consequence, an alteration in its functions, in other words, disease. Whether or not such a disease will be evident to others will depend upon the extent and gravity of the change. A slight deviation will give rise to no detectable symptom; and in such a case will exist as a pre-disposing cause. Such a state is called dormant disease, which represents an amount of change in the molecular condition of a part that is not incompatible with the continued performance of the concrete function of the organ, but which will, nevertheless, endow the part with a property not possessed by the healthy germinal matter, viz., inability to resist some stimulus which, if applied to the healthy part, would have had no injurious effect. Like one piece of machinery with the cast-iron where wrought-iron should have been, it will do its work as if all were right, until a strain comes in a certain direction, when the cast-iron will snap, and the integrity of the machine be destroyed. In this case the improper molecular construction of the iron is the pre-disposing cause, and remains dormant in the absence of the strain. The strain itself is the stimulus (exciting cause) of the disaster which results from its application. Whether such a deterioration will be transient or permanent must depend upon two causes. 1st. And chiefly, according to the continuance or otherwise of the mal-assimilation, because so long as such a part is supplied with ill-formed pabulum

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it cannot regain its health. 2nd. According to the strength of the tendency in the part to re-produce its like, or to revert to its former condition; when the latter is strong, if really good pabulum is supplied, the new and diseased property will soon be lost, if the former, a result, most probably, of the duration of the deterioration, then, even with the most healthy food, a considerable time may elapse ere the part reverts to its norm.

These general principles may be applied to the explanation of every form of disease; the one condition which underlies them all being a change of motion in the germinal matter in response to some unusual stimulus.

Diseases thus considered are just as natural results of the altered circumstances, as health is the natural result of the original circumstances in which the system was placed; or, in other words, pathology is the physiology of parts in unusual circumstances.

The same considerations prove that there cannot exist any true resistance to disease, or any real *vis medicatrix naturæ;* all the changes which follow the application of stimuli being the direct and necessary results of the pre-existing molecular constitution of the part. Germinal matter must be said to act blindly; and to be just as ready to perfect a self-destructive act as to bring about a change which may tend to throw off the disease. In fact it cannot be imagined as exercising any choice in the matter. It is as much a machine as any dead piece of mechanism.

The well-known tendency of a large number of diseases towards recovery is traceable to the *inherited properties of the germinal matter;* and just as in certain families we meet with inherited tendencies to certain diseases, and at once trace them to peculiarities of molecular constitution of the parts concerned, so in the majority of cases the tendency of germinal matter to resort to its original mode of motion, after it has been turned out of its normal course, is sufficiently strong that most acute disturbances speedily and spontaneously come to an end.

Diseases are grouped together according to resemblances in their origin, their locality or their course; but for the purpose of determining what kind of treatment will be best adapted for their cure, we must ascertain what the stimulus is which has roused into activity the abnormal properties of the germinal matter implicated in the morbid process.

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I think we are now prepared to answer the question as to how diseases ought to be treated, and also to test the propriety of many of the means of cure most frequently resorted to.

As during all the manifestations of life the three essential constituents are germinal matter, pabulum and stimulus, we must attend to all these in our endeavours to remove disease. The study of the changes of germinal matter constitutes pathology; the study of pabulum, dietetics; and the study of the action of stimuli includes the whole field of drug-treatment. We will, however, begin with the last point, as most germane to our subject.

Every true drug acts as a *stimulus*, and the symptoms it produces are the evidence of this action. Now, according to our explanation of the action of stimuli, it must follow that drugs will act very differently, according to the pre-existing condition of the matter to which they are applied. For example:—

1. Before a drug can act chemically, it must have reduced the substance upon which it acts to the chemical degree of molecular constitution, or, in other words, drugs cannot act *chemically* on living germinal matter; they must kill it first, and having thus split it up into chemical compounds, they will combine with these according to their wont. This is a very important fact, as showing that unless we desire to destroy a part by chemical agents, as with caustics, we must not attempt to prescribe them as acids or alkalies, or indeed in virtue of any of their chemical properties, unless we can limit their sphere of action to dead matter. We may justifiably employ chemicals to modify the condition of unabsorbed food, or to secretions after their formation; but to expect a chemical action within the metabolic sphere, is to look for an impossibility and to reap a failure. The sphere, therefore, of chemical treatment is outside the living matter. 'Tis no doubt true that many invaluable drugs are also powerful chemical agents; nevertheless it is capable of demonstration that they influence germinal matter, not in virtue of their chemical affinity, but in virtue of their power to stimulate into activity some pre-existing property of the germinal matter itself. It is on this account that chemical substitutes can never be used in true scientific drug-treatment. If, for example, an antacid were required, the practitioner might use any of the alkalies or alkaline

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earths, and if to neutralise the acids of dyspeptic fermentation were the only purpose for which it was required, the substitution of one base for another would be unimportant; but since every alkali and alkaline earth has its own peculiar power to stimulate germinal matter, the use of each one is apt to be followed by its own specific effects, over and above the chemical action upon the acids of the decomposing food. In the stomach they all act as alkalies, and neutralise the acid with which they come into contact; but beyond the stomach, in *addita* of the body, potash acts as potash, soda as soda; and nothing short of an accurate foreknowledge of these specific effects, and a judicious avoidance of overdoses or protracted usage, can prevent a physician from doing infinite damage by an attempt to rectify a chemical fault by a chemical corrective.

2. A drug cannot act as a specific stimulus unless the germinal matter possesses the property of responding to its peculiar mode of impression; and as the germinal matter of different organs differs essentially in its properties, it follows that every different substance will modify the changes of germinal matter in different parts or in different ways. We may expect, therefore, to find that every substance capable of modifying metabolic changes will have what looks like an elective affinity for certain organs; or in other words, a drug, though brought into contact with germinal matter in all parts of the body, will initiate new modes of motion in certain parts only.

3. A drug which is capable of acting on a given part of the body in a certain way, as long as the molecular movements of that part are normal, may act in a totally different way upon the same part, if a new set of movements are going on. In other words, the action of a drug upon a healthy and a diseased organ may differ to any extent.

4. In like manner, a drug may be perfectly incapable of acting on a certain organ while it remains healthy, and yet be capable of modifying to a great extent any morbid changes which it may be undergoing.

All these variations are the natural conclusions deducible from the principles we have laid down, and experience has abundantly proved every one of them.

Such being a general outline of drug-action, it follows as a necessary consequence that no drug can be used judiciously as a means of cure, until we have become

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acquainted with its powers of influencing the healthy functions. Unless we know accurately what parts of the living germinal matter it will influence, and also what changes it will inaugurate, we cannot prescribe it with precision. A proving of a drug on the healthy body, however, will be insufficient, since we have seen that it may and probably will act very differently on the same part when diseased; and hence we must ascertain this point also. It is at this point that Hahnemann meets us with his invaluable discovery, viz., that a drug invariably produces in the diseased organism a series of changes precisely opposite to those which it produces in health, and that consequently it removes from a diseased part the entire series of symptoms which it would excite in the same part when healthy; and hence his law for the selection of a drug is "*Similia similibus curentur.*" We shall return to this, but in the meanwhile must proceed with our enquiry as to how the treatment of disease should be regulated by the light of the most advanced physiology.

Bearing in mind that germinal matter alone is truly living, and that every substance found in the body is produced by this same protoplasm, we must not expect to supply defects in formed material by supplying it direct —everything that enters the stomach is changed into the metabolic state before it can become part of any formed material, whether structural or not. Every oil-globule found in the cells of fat or elsewhere has been produced by germinal matter, and in no instance consists of oil which has been simply absorbed and re-deposited. So also with albumen, fibrin, gelatin, *et hoc genus omne;* not one of these can be made to enter directly into any of the structures of similar composition; each must first lose its identity, and, emancipated from all its dead chemical property, must enter the metabolic state, and thence be reproduced by deposition as formed material. The practical lesson to be learned from this is, that chemical analysis of food, as far as proximate principles are concerned, will be of no assistance whatever in regulating the diet. The relative amount of albumen, fibrin, gelatin, oil, &c., tells us nothing, as all these cease to exist before true nutrition becomes possible. Ultimate analysis is useful, as we know that the body must receive from without all the carbon, oxygen, hydrogen, nitrogen, calcium, ferrum, phosphorus, &c., which are found in its formed materials; but,

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beyond the important fact that the compounds of these must be in the colloid condition, the superiority of one form over another as food is regulated by other proportions than those deducible from chemical analysis. Their physical forms, as influencing the facility of their disintegration; the effect they produce on the gastro-intestinal mucous membrane; the more or less readiness with which they are absorbed, &c., are all of vastly greater importance than anything we can discover by chemical re-agents. Hence a chemical diet-table, treating of proximate principles, is among the most misleading of all the so-called helps to the treatment of disease or the maintenance of health.

If all diseases consist in changes in modes of motion of germinal matter, the converse must likewise hold true, and all changes in the modes of motion of germinal matter must be looked upon as disease. *Hence true cure must in every case consist wholly and solely in a restoration of the original mode of motion to those portions of germinal matter which are diseased;* and any benefit which may be derived in a cure of disease by producing changes in any other part of the body, must be considered as *indirect*, and the process regarded as *spoliative.** One would imagine that such a statement as this would be hardly necessary, as it must be self-evident to all who think calmly over the subject; and yet it is all but ignored in practice, and the only system which holds it up as a standard of comparison and adheres to it by the sick-bed is denounced as unscientific and absurd. To restore changed action, by directly influencing the diseased part, is the very essence of specific treatment, and Dr. Drysdale has admirably defined a true specific to be "a remedy which cures by the absorption of its whole physiological into its therapeutic action;" or, paraphrasing this to suit the point from which we are at present viewing the question, a specific is a remedy whose therapeutic effect is to restore

* I do not by any means wish it to be understood that the spoliative treatment should never be adopted. I know well that in many chronic diseases the system is loaded with material of a low degree of vitality, of which it should be relieved as an important step towards cure. But even here a diet so arranged as to compel the system to work up and work off its bad material is infinitely to be preferred to any attempt to effect the same result by disturbing doses of drugs.

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the normal mode of motion to the disordered germinal matter, and whose physiological effect would have been to have produced precisely the same disordered action, had it not already existed. The one effect exactly balances the other, and the practical result is that there will be no evidence of medicinal action at all; the cessation of disease and the restoration of health are the only consequences of the administration of the drug. This is the *beau ideal* of treatment. There is no spoliation—no waste of force; the existing error is corrected, and all goes on as if disease had never been.

Let us now enquire how far the dominant school of medicine follows this course. At the very foundation of almost all their treatment lies the error, that they give remedies *to do something* in place of to *undo*—they use medicines to produce their direct action, or in other words, to *produce a morbid change*. Now this must be wasteful and spoliative, and can only be defended on the ground of expediency; it is literally at best doing evil that good may come, and unless it can be distinctly proved that there is no other known way, it cannot be recommended. No wonder that many thoughtful practitioners have come to the conclusion that it is better to leave diseases alone, and to trust to the well-ascertained "tendency to revert to the original type" to correct the errant action of the disordered part; and so it would be, if spoliative treatment were the only possible alternative. We, however, deny the necessity for this. We know that drugs properly chosen will correct existing derangements, and without producing new ones; and this, we reiterate, is their proper function.

Take an example. *Colocynth* has been proved to act on the bowels, and to produce colic and diarrhœa. The dominant school use it, accordingly, where they wish to empty the bowels, and give it in a dose capable of producing its physiological or pathogenetic effect; but not wishing to induce colic as well as purgation, they add *hyoscyamus*, which experience has taught them counteracts that part of its effects; and having got rid of the colic, they are well satisfied with the result.

We, on the contrary, give *colocynth* where colic and diarrhœa already exist, and, without the aid of *henbane* or any other adjuvant, the colic and diarrhœa both cease, and the patient is restored to health. The allopath, how-

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ever, has given the remedy because he wanted to produce an over-action of the bowels, and accordingly *colocynth* used in our way would not suit his purpose; unless, indeed, it was a case of constipation associated with colic, in which instance *colocynth* will frequently cure the one, and in so doing overcome the other, which is often a mere consequence, and does not own a separate origin. If, however, no colic exists, he must ascertain the exact condition of the intestines upon which the constipation depends, and acting upon this knowledge, select the proper specific to correct the error. To give an aperient under any circumstances, is contrary to the principles we are now defending, because it is producing a diseased condition. To restore the natural action of the bowels is quite legitimate, but to produce an overaction must be spoliative, and hence to be avoided whenever it is possible. To revert to our mechanical illustration, what workman in his senses would try to correct the wrong action of one machine by making another machine act wrongly, even though the second disturbance should in some measure counterbalance the first? Unless it were the only means of preventing the destruction of more or less of the machinery, such a step would be unwarrantable, and certainly could not be defended so long as a direct correction of the original disturbance were possible. What has been said against aperients may with equal justice be said against all other developments of the physiological action of drugs; and thus we arrive at an important point regarding the dose of a drug, which, according to these views, should always be too small to produce its physiological effect. I shall not, however, enter into this subject, as it will form a special question for discussion in Dr. Black's paper, to which we are all looking forward with interest.

A careful survey of the therapeutics of the most advanced practitioners will demonstrate the fact that drug administration has become more and more limited chiefly to three classes, viz., sedatives, tonics, and specifics. Of specifics such as Iodide of Potassium, Quinine, &c., I shall say nothing, since in most of these the relationship between drug and disease is of the kind we are advocating; and any question we may still be disposed to raise has reference rather to the mode of administration than to the art of selection.

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Of the three classes, however, that of sedatives is the one most frequently abused. Nothing seems to me more astonishing than the manner in which the most advanced physiologists recommend the use of sedatives; and the insane rush after every new pain-killer by practitioners of all classes shows clearly how little a correct physiology has hitherto regulated the practice of the profession. Upon what principle this unscientific use of narcotics is defended I am at a loss to conceive, unless it be that, recognizing their inability to exert any direct effect on the progress of the disease, they think they will, at any rate, render that progress as comfortable as possible to the patient. Just as if the well-wishers of France should have cut the telegraph-wires to prevent the Assembly at Versailles knowing anything of the doings of the Communists in Paris, and have defended the act as a wise precaution lest the deliberations of that august body should be disturbed by the exciting nature of the news brought to them. I am well aware that much has been said of the advantages of physiological rest, and of the exhausting effects of pain; but let it be remembered that I am not arguing against the removal of pain, but against the prevalent method of achieving the result; neither does it appear to me possible to conceive anything less *physiological* than the rest obtained by inducing a morbid state of the morbid centres. Who, in his senses, believes in a really innocent narcotic? What were we so confidently told about Bromide of Potassium and Chloral Hydrate? Were they not both lauded as "blessings to humanity," and perfectly harmless removers of pain and sleeplessness? and are not the medical Journals already teeming with warnings of their dangers, or at least their many "inconveniences"? I was much pleased, and not in the least surprised, to read the following report of Dr. Maudsley's opinions in the *Lancet* of August 12th, 1871. It is as follows:—

"Dr. Maudsley began by seriously doubting if it were always a wise thing to stifle excitement; and whether a chemical restraint put upon the brain-cells was not often as injurious to the patient as a mechanical restraint imposed upon his limbs. He thought that sedatives were given far too recklessly; that, although they might relieve symptoms, they often only served to push the patient further down the hill, and, as often as not, re-

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tarded recovery. He thought that the whole range of sedatives, including Bromide of Potassium and Hydrate of Chloral, were equally capable of being abused; that by giving them we often seriously damaged the patient's general health; and, instead of curing, we often merely 'made a solitude and called it peace.'"

These are words of sound wisdom which I trust will bear practical fruit. Dr. Maudsley is too sound a physiologist and too clear a thinker to be long misled by so deceptive and unscientific a procedure as that he comments on. Surely the time must soon arrive when physicians will learn that they have a higher mission than that of putting disease out of sight; that it is a wiser and better plan to look to the future; to let the patient suffer pain on the way towards health rather than enjoy present relief at the cost of drifting into hopeless disease.

It is very lamentable to find an accomplished physician like Dr. Anstie claiming, as among the glories of our recent advances in Therapeutics, the subcutaneous injection of Morphia, and the employment of Bromide of Potassium and Hydrate of Chloral.

So long as the fundamental error of producing a new disease for the purpose of curing one already existing—no matter whether the new morbid action is set up in the part already diseased, or in some more distant organ—so long as practitioners will give drugs in disturbing doses, so long no real progress in drug treatment is possible. Either the entire facts of our advanced physiology must be interpreted differently, or the very foundations of old-school drug-treatment must be re-laid.

Physiological experiments with drugs will continue absolutely barren of therapeutic advantages while their results are utilized in so erroneous a manner. If a remedy is found experimentally to paralyse the motor ganglia of the spinal cord, of what possible use can this knowledge be if the remedy is to be given in paralysing doses? What true physiologist would recommend the production of such a serious morbid condition as a method of cure? When, however, drugs are employed rationally, that is, specifically, such a knowledge becomes invaluable, since upon the ascertained principle that drugs produce the exactly opposite effect in disease to what they do in health, we know that such a drug, in a proper dose, will remove paralysis of the cord, provided the restoration of

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its normal mode of motion has not been rendered impossible by an entire change in its constitution, or, in other words, provided disorganisation of the cord has not already taken place.

If time permitted I might show that the fashionable methods of treating disease by stimulants and high-feeding have both been carried to an extreme which is opposed to all the teachings of physiology; but here, the error being simply one of degree, it will probably right itself; and I have therefore preferred confining my attention to drug-treatment where the errors are fundamental, and where the change must be thorough to be of any real value.

Let physicians once realize to themselves the true nature of life, of health, and of disease, and they will never commit the grave error of producing one disease for the cure of another, except, indeed, under protest, and as a matter of expediency.

Let them once recognize the true nature of drug action, and its specific relation to diseased action; and they will receive a light wherewith to guide them through the difficulties and intricacies of a consistent therapeia.

Let them be content to follow and help the natural course of diseases rather than force a method of their own imaginings upon a blindly-acting machine which can offer no resistance, and may therefore be led to destroy itself; and they will cease to prescribe drugs to produce disturbances, under the idea that they can in this way drive the pre-existing disease to a happy termination.

And, finally, let us rejoice that every real step in the progress of modern physiology has strengthened the scientific foundation of specific drug treatment; and that we are able to stand abreast with all the workers in this vast field of research, and, accepting with deep thankfulness each new discovery, are in a position to render it useful at the bed-side of our patients.

Gentlemen, I will now conclude with hearty congratulations as to the position of homœopathic treatment as established by the most recent advances in physiological research.

Sackville-st., W.

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ON POSOLOGY.

By F. Black, M.D. Edin.

Under no circumstances is the remark more true, *ars longa sed vita brevis*, than when applied to all that tends to the finding, and giving of a remedy. The truth becomes still more evident when such a subject as the general question of dose has to be introduced, and discussed within the limits assigned to this Congress. My endeavour shall be to present the question to you in as concise a manner as will enable you to express an opinion as to the fitness of the manner in which I recommend the subject to be investigated. It is much easier to ascertain the dose of a drug which is given to exert its physiological than to fix the necessary quantity to excite its specific curative action, *e.g.*, it is easy to determine the quantity of *colocynth* necessary to procure purging, but it is more difficult to ascertain the dose which will cure colic, and diarrhœa.

To guide in such an experiment, Hahnemann, in his first *Essay on a New Principle for Ascertaining the Curative Power of Drugs*, has given a clear direction:— "The cautious physician, who will go gradually to work, gives the ordinary remedy only in such a dose as will scarcely perceptibly develop the artificial disease, and gradually increases the dose, so that he may be sure that the intended internal changes in the organism are produced with sufficient force, although with phenomena vastly inferior in intensity to the symptoms of the natural disease; thus a mild and certain cure will be effected." Following this very sound rule, Hahnemann is found from 1790, when he made his remarkable experiments with *cinchona bark*, to give for many years the specific remedies in ordinary material doses. Then he gradually reduces the quantity, in order to avoid the overaction of the drug, and also to present it in a more suitable form. He first intimates his recourse to minute doses in his paper on Scarlet Fever, published in 1801. The dose of *bell.* he there recommends is somewhat between the 2nd and 3rd centesimal dilutions. It is not to be wondered

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at that a dose so minute excited sceptical astonishment, and that in the same year Hufeland called on Hahnemann to explain the action of these novel quantities. Hahnemann's answer was most unfortunate, and it has launched his system of therapeutics into a sea of troubles, where a great truth has to struggle against currents into which it is steered by its own adherents. "You ask me," he answers, "what effect can the millionth part of a grain of *belladonna* have? I hate the word *can;* let us ask nature what effect it has." He gives no account of the steps or reasons which led him to arrive at these minute doses; what they were is mere matter of conjecture; but he simply states that though a hard pill of *extr. bell.* produces no effect on a robust labourer's stomach, it by no means follows that the same dose be a proper or too weak a one if the same man were ill, or if the grain of extract were given in solution. He goes on to say, "it is only by stirring, by brisk, long continued stirring that a liquid medicine obtains the largest number of points of contact for the living fibres, thereby alone it becomes right powerful." Then he makes one of those exaggerated statements which too often mar the truth in his posology. "A teaspoonful of this mixture (containing the millionth of a grain) brings the robust labourer to the brink of the grave if he was previously ill, and if his disease was of a description such as *belladonna* is suitable for." (p. 443.)

And now Hahnemann enters on the mythical stage of posology; he entertains certain fancied views, and these grow into assertions. He forgets the solid rule given in 1790, and now, struck with the power of infinitesimal doses of well-selected specific remedies, he enunciates hypotheses as dogmas, and thereby exposes his great discovery and his wonderful genius to bitter opposition, and not always unmerited ridicule. He does not rest satisfied with the fact that a millionth or a still smaller quantity acts, but his mind is possessed with the idea that the causes of natural disease are conditional, but the operation of medicines absolute; that there is no medicinal substance which, when employed in a curative manner, is weaker than the disease for which it is adapted; that scarcely any dose of the selected homœopathic remedy can be so small as not to be stronger than the disease. These are his views in 1805, and they are the cause of his excessive, and always increasing diminution of the dose.

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He also expresses his belief in the spiritual character of medicines; but it is not until 1827 that the processes of dilution and trituration, which he at first used as modes of systematic division, became in his opinion agents of great power, producing a hitherto unknown, undreamt-of change—the setting free of the dynamic powers of medicines. These medicinal virtues, he holds, may be potentized almost to infinity: this is his theory of dynamization —a process of unveiling and vivification of the medicinal spirit.

Time will not allow me to enter on the arguments which science opposes to an hypothesis founded on the error that matter and properties are separate entities, and I shall now merely give a few illustrations of its curious practical manifestations. You are all familiar with Hahnemann's remarks on the destructive power of potentized *drosera*. But while Hahnemann warns against giving *drosera* more than 20 shakes, Jenichen, the enthusiastic manufacturer of high dilutions, advocates *drosera* subjected to 60,000 shakes! Hahnemann took to globules as a protection against the power gained by shaking liquid medicines He warns those who carry about with them in their pocket-cases medicines in a fluid state. "I dissolved," he writes, "a grain of soda in an ounce of water mixed with alcohol, in a phial which was thereby filled two-thirds full, and shook this solution continuously for half an hour, and in dynamization and energy it became equal to the 30th potency."*

If this be the case, why all the labour of making dilutions?

This dynamization process, according to Hahnemann, overrules all chemical laws; for a globule of *phos.* 30, exposed for twenty years, is still said to yield to olfaction all the curative effects of pure phosphorus.

In 1832 he holds the idea that the higher the dematerialization of the drug is carried the more penetrating and rapid does it become, and that one medicated globule by shaking with others affects them by a kind of infection. Dr. Gross, his enthusiastic disciple, affords the *reductio ad absurdam* when he asserts that he communicated blood-power to thousands of globules by shaking them up with one dry globule of his own blood, and with these he effected wonders in congestion!

* *Organon*, note to § cclxx.

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I have dwelt on these peculiar views of Hahnemann because they have had a most injurious effect on specific posology, and because they have been, and still are, a constant provocation to the medical world which differs from us. All may admit that the division of many medicinal bodies places them in a better condition to develop the finer and more specific effects of the drug; and what increases physiological action also tends to render the therapeutical administration more suitable, by presenting the medicine in such a dose that it shall most certainly act—to gain the maximum effect with the minimum quantity. But these admissions are very different to Hahnemann's ideas of dynamization. Unfortunately, his views, though not adopted in their entireness, have injuriously leavened too many of the homœopathic school, by communicating to its posology a vague, unlimited character.

How, then, is our posology to be placed in a position which may be regarded as scientific, and bring us more in unison with the great mass of the profession? By removing it from a pharmaceutical foundation, and basing it on the one side on the physiological action of the medicine, and on the other on the susceptibility of the patient. These bases do not work out any exact formula which assigns a particular dose of a certain medicine in a certain disease, for there are several varying and disturbing factors which ever present themselves; but they afford principles which have this great advantage, that they restrict the limits of choice and afford marked landmarks, whereas Hahnemann's teaching is without a track, vague and boundless, and presents no principle which admits of general application. The directions Hahnemann first gave in his first essay, to administer the remedy cautiously, so as to gently stimulate the diseased part, and as needs be gradually increase the dose, is founded on a physiological basis, and the physiological action of the drug has from time to time been recommended by several as a guide in posology.

In a paper which I read to the British Homœopathic Society in 1847, I proposed that as in the provings of medicines symptoms differing in kind and degree arise from the manner and doses in which the drug is given, the proving will afford definite grounds for choosing the dose; and I illustrated this from the full proving of

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colocynth. But I soon found that this, though good as a principle, could not, from the imperfect state of our provings, be worked as a definite rule in choosing the dose. The same objections apply with greater force to the rule of Drs. Hering and Hale of America. They consider the course of the medicine-disease must correspond to that of the disease to be cured; that having chosen a remedy, the point is then to ascertain whether the symptoms correspond with the primary action of the drug, in which case the lower dilutions are to be given, or with the secondary, when the higher are to be given.

The physiological basis was lately ably maintained by Dr. Yeldham, and his view may be expressed in the formula, the therapeutic action lies very near the limits of physiological action.

Add such a principle to Hahnemann's first rule for giving just sufficient to stimulate the diseased part, and the choice of a dose becomes simplified.

Specifics are "medicines whose whole physiological action is absorbed in their therapeutic effects;" so that a dose falling somewhat short of the physiological is sufficient to cure.

It may be safely asserted that all the physiological actions of a remedy can be gained by doses confined within the 3rd decimal dilution. It may be objected to this that many of the symptoms inserted in Hahnemann's Materia Medica are the result of provings of the 30th. But if even one-half of these subjective symptoms have any firmer basis than the imagination of the prover, there is no evidence that they could not have arisen from much more material doses.

The practical difficulty in giving such a dose as will simply stimulate to healthy action, without manifesting physiological or chemical symptoms, is the varying susceptibility of the patient, and the peculiar influence of the disease under which he suffers. Into this I do not enter, further than stating that the greater the susceptibility, so the less is required of the specific stimulant. Expressing my own experience and that of many men of extensive practice who have looked at the dose with a judicial eye, I believe all the requirements of posology, as expressed in the physiological action of the drug and the susceptibility of the patient, are met within the limits of the 3rd dilution.

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If the requirements of practice can be met within these limits, the great gains are very evident.

1. The gains in a pharmaceutical sense are enormous.

2. Such limitation gives more time to examine the varied character of medicines as regards their doses, and more attention can be paid to the very important point of repetition.

This subject of the dose has lately been several times discussed by many of you. I shall not therefore further occupy your time, except in giving a few cautions which are necessary, in order to restrict the discussions to the actual question.

Let there be no misunderstanding as to the question I put. I have no desire to dogmatically confine the limits of the dose. I do not deny the curative power of medicines above the 3rd, but I maintain there is no evidence that they act better than those below the 3rd; and I invite those who are in the habit of using the higher dilutions to make the experiment which I for years have made, especially with such medicines as *sul.*, *sep.*, *lyc.*, *graph.*, *carbo*, *cham.*, the employment of which has been much influenced by Hahnemann's later teaching. Also with the action of such as act energetically, such as *ars.*, *acon.*, *merc.*, *bell.*, &c.

While investigating the dose, it is needful to remember the altered circumstances in which we stand as compared with the early periods of homœopathy. That was a time when an allopathy prevailed so violent in its resources, that the withdrawal of these had often a speedy and wonderfully happy effect on the patient who sought homœopathic advice; so that some considerable deduction must be made from the cures of that period. In the present time, the increasing knowledge of the natural course of disease calls for much more stringent proof of the action of a remedy, than what was formerly deemed sufficient to distinguish recoveries from cures.

If moderate proof is sufficient to establish the efficacy of material doses, greater proof is required for the action of fractional; and the evidence needed to prove the value of infinitesimals mounts in a quickly increasing ratio, as you pass from one dilution to another.

Clifton, Sept. 1871.

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ON OVARIAN AND UTERINE DISEASE.

By Dr. JOHN MOORE.

I PURPOSE in the present paper to give a few facts observed by me in the treatment of ovarian and uterine disease during the last twenty years; and afterwards to draw a few inferences which will place the whole subject before us for discussion, with a view to ascertaining what class of cases it is in which surgery is required in addition to medicine, whether homœopathic or otherwise, administered internally; and also what is the nature of those for which homœopathy, pure and simple, suffices for the cure. The great practical importance of this subject and its attendant difficulties, seem to point it out as one appropriate for discussion at our meeting here to-day.

It was brought forward at the first Homœopathic Congress held in this country, at Cheltenham, in 1850, by Dr. MADDEN, who read a very able paper *On Uterine Disease,* detailing his experience at the Brighton Dispensary, which comprised reports of 180 cases. This paper was subsequently published in *The British Journal of Homœopathy* for January, 1851. Much discussion arose upon it; and much difference of opinion was expressed as to wherein consisted the use and abuse of caustics in the cure of ulceration of the os and cervix uteri.

One year afterwards Dr. LEADAM published ten cases in *The British Journal of Homœopathy,* which were all cured without escharotics; though local applications, such as injections of *calendula,* &c., &c., were used in addition to purely medicinal treatment.

The editors, commenting upon both papers, regarded them as inconsistent with the strict letter of the *Organon;* but of the two, Dr. Leadam's as the most inconsistent, though—it must be admitted—the mildest. Thus stood this vexed question twenty years ago; and the same differences of opinion and practice still exists to some extent in this country; but chiefly among homœopathists in America, where the high dilutionists assert that they

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effect cures of uterine disease without escharotics; while a large number of homœopathic physicians employ the surgical application in addition to the purely medicinal treatment.

It will facilitate our enquiry and discussion if we bear in mind that the subject embraces three classes of disease.

1st.—Those in which the ovaries are chiefly or solely affected. (Pure ovaritis.)

2nd.—Those in which the uterus is the seat of disease.

3rd.—Those in which both ovaries and uterus are involved; and which may be styled the complex class, comprising as it does those cases which are the most difficult to meet; and which need much careful examination before we are able to ascertain the right process of cure.

Though complex cases do occur, on close examination it will be found that one organ is mainly and primarily affected, and the other secondarily and slightly; and the order of the plan of treatment must be the same. I shall now proceed to cases illustrating these statements; and I shall begin with those which are strictly *ovarian*. One case occurred in a married woman between 35 and 40 years of age, of fair complexion, and highly nervous temperament. Had never completed the full term of pregnancy; having twice miscarried between the fifth and sixth month, or a little later. It was on the occasion of her last miscarriage, or premature labour, that I was sent for. As soon as she had sufficiently recovered from its effects, I resolved to find out the cause if possible. Though most anxious for a family, she declined examination *per speculum* on the ground that Sir Chas. Locock and Dr. Ferguson had both examined her, and pronounced the uterus to be perfectly sound; and that debility alone was the cause of the premature labour. This reason was not conclusive, because there were no indications of very great debility. On going closely into the symptoms I found pain in the region of the left ovary, extending down the inner side of the thigh. From some previous experience I thought of *phosphorus* provings, which show it to have great influence on the sexual organs, both male and female, by its acting violently on the testes and the ovaries, producing profuse menstruation even after it had ceased for eighteen months; by various kinds of leucorrhœa—chiefly the reddish—&c.

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On October 29th, 1850, I accordingly prescribed *phosphorus* 6 for a few days, thrice a day. She complained that the medicine irritated her, though she thought it did her good. I now gave two or three doses of *chamomilla*, and retained the *phosphorus;* but then gave No. 30. At my next visit she still complained of the medicine irritating her, though she felt the pain better. I now gave her *phosphorus* 100 (Jenichen), a dose only once in twenty-four hours. After this I heard no more of the irritation, but she steadily improved; and I continued this for about eight weeks—the only intercurrent being *nux;* and on one occasion *mag. mur.* for a few days—for what reason I cannot remember. I returned on the 3rd February, 1851, to *phosphorus* 30. In March she was quite well, and on the 30th March, 1852, one year afterwards, gave birth to a son, born at the full time.

The other case to which I desire to draw your attention is that of a married woman, æt. 40, who I was called to see in October, 1866. She was five-and-a-half months gone in pregnancy. On visiting her I found her threatened with a second miscarriage. The characteristic pains had commenced; but *no* hæmorrhage had appeared. By aid of *belladonna* and *sepia* it passed off. On inquiring into the causes I found each ovary increased in size to the extent of a large hen's egg; and it was a matter for astonishment how she could have become pregnant in such a state. I gave a very doubtful prognosis as to her being able to complete the full time under such circumstances; but advised her to undergo a course of medicinal treatment. There was no indication whatever of uterine ulceration, or of any form of disease of that organ. Regarding the ovaries as chronically inflamed, I prescribed *bell.* 1, *mercurius sol.* 3 and 3x, and *bryonia* 1 for a few weeks. She went on to her full time, and had a living child, which she nursed. She felt such slight inconvenience that she paid but little attention to herself; but after weaning she became anxious to be cured of her chronic disease, in order to avoid a repetition of her former troubles. She now suffered great pain at each monthly period, the return of which had been regular. Examination of the uterus did not discover any disease of that organ. Both ovaries were distinctly enlarged. By persevering in the line of treatment I shall presently describe, one (the left) has disappeared, and the right one is still slightly perceptible by

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deep pressure, and apart from pressure, she suffers pain in it occasionally. She has never become pregnant again; and four-and-a-half years have elapsed since the birth of her only child.

The medicines I prescribed separately were *mercurius sol.* 3 and 3x, *bryonia* 1, *kali bromid.* ℗, *k. hyd.* ℗, and *iodineum* ℗, the latter dissolved in *kali hydriod*, thus, *iodine* gr. 1. *k. hyd.* gr. x. in ℥ viij. aquæ, *cochl. min.* bis in die sumend; thus the dose of *iod.* became the 64th of a grain, this I continued for weeks with such intercurrents as appeared to be required, *e. g.*, *merc.*, or *puls.*, or *nux.* I also used a liniment of *iodine*, ʒ i. to *Ol. olei.* ℥ i.

The Treatment of Ovaritis.

Remembering the various tissues entering into the structure of the ovaries and their appendages, viz., serous, mucous, and ligamentous membranes, together with the parenchyma, we are prepared to expect a variety of symptoms, some of which are very painful. Our medicines must be chosen here, as elsewhere, in harmony with the totality of the symptoms. Though highly and purely nervous symptoms are often met with, yet we should be careful not to set down all, or indeed many, of the symptoms to neuralgia. The ovaries being, according to my experience, very frequently inflamed, or congested, when the derangement is styled neuralgic, I feel sure that if we treated such cases as inflammations, we should obtain more brilliant results. Note also that every month the ovaries and uterus undergo a natural congestion; and if not relieved by the proper secretions, there is laid the foundation for disease after an epoch of time. The kind of pain will point out the most homœopathically selected medicine; and these will be *aconite*, or *belladonna*, or *bryonia*, or *mercurius*, *phosphorus*, *iodium*, *pulsatilla*, *platina*, *colocynth*, *lachesis*, *conium*, *staphysagria*, or *cimicifuga.*

As regards the *indications* for the medicines I suggest the following: When the ovarian pain shoots or extends towards the hip—*mercurius*, *bryonia.* When upwards to the side—*pulsatilla*, *cimicifuga.* When the pains shoot down the inner side of the thigh, *phosphorus* is most reliable; perhaps also *staphysag.* and *colocynth.* In bilious headaches, &c., &c., *gelseminum.* When numbness in the limbs, *platina* and *colocynth.* When accompanied by

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great debility—*china;* but if not presumptuous, I should like to distinguish between those medicines which relieve, and those which cure. Chronic ovaritis has often been cured in my hands by *belladonna, mercurius, bryonia,* and *phosphorus.* The great difficulty is tiding the patients well over the monthly period, as that is usually a time of aggravation; and we shall now, therefore, say a few words on dysmenorrhœa, and its treatment; because it is intimately allied to the treatment necessary for ovaritis at the monthly periods; and is of itself sufficiently difficult to treat to merit especial notice. Some cases cause intolerable suffering every month; and very little can be done by the old system save by overpowering narcotic doses, the effect of which is such that the patient will rather bear the disease than its prescribed treatment.

Dysmenorrhœa arises from three different causes, hence we have the neuralgic, the congestive, the mechanical; but in practice the three are often so blended that it is not easy at first to discriminate which feature is predominant. Perhaps for all practical purposes the congestive and neuralgic may be considered together. When the pain sets in with violence in the ovarian regions, and through to the back; and is followed by profuse secretion, and even hæmorrhage, which often happens, we may look upon it as a *congestive* case.

If, on the contrary, the pain runs down the thighs, or even as far as the feet, we may regard it as *neuralgic.*

When, on the contrary, there is great pain, and bearing down with *forcing* feelings in the *uterine region,* and very *little* discharge the first or second day, we may pretty certainly conclude that the case is a *mechanical* one; arising from contraction of the inner and outer os and cervix uteri, probably of both.

There are 90 medicines in Guernsey's work marked for dysmenorrhœa! One is puzzled how to use such wealth. In *practice* you will not find many of much use, and in some bad cases it is very difficult indeed to find one.

Belladonna, cocculus, colocynth, platina, arg. nit., conium, gelseminum, phytolacca, and last, but best of all, *senecio (aletris?),* are the most reliable, and, in cases where these fail, *kali bromid* ℗, *kali hydriod. Kali nitricum* will be found of great service, taken at the intervals of the period, giving *senecio, gelseminum,* or *bellad.* at the time.

When all medicines fail, and the symptoms point to

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mechanical obstruction, we are justified in proposing examination, and dilating the os uteri with the sound. It is only in rare cases of this kind that tents are really required.

In 1868 I had a very severe case of this disease under my care, in a young person aged *23*. All the three classes of symptoms were present. Not only was there pain in the ovaries, but it extended down to the very feet, and violent pressing pains in the uterus during the first two days of the period. I could not with any medicines give her relief; so I proposed an examination, and found the organs very small, os contracted, and cervix likewise. With some trouble I passed the sound twice or thrice the week before the period. This gave some relief, and removed the intense irritability of the organs; but I attribute the cure to *kali bromid* ℗, which I gave during the interval of the monthly period, and *senecio* at the time of the attack. Doubtless the great difficulty in the great majority of cases is the separation of clots instead of the natural secretion; and dilatation gives relief by allowing these to pass easily; but I am convinced, by experience in this and other cases, that the majority of such cases may be cured medicinally, and this plan is always to be preferred, even though it may be a more tedious process.

Though I have endeavoured to map out the various kinds of dysmenorrhœa, and to discriminate the mechanical from the others, I do not wish to overlook the great fact that the dynamic symptoms are so often intermingled with the mechanical that great benefit may be derived from our medicines, even if the mechanical obstruction exists. But for the perfect cure, *dilatation* is necessary.

I had here intended to lay before you the details of several cases of uterine disease, in which surgical treatment has been found necessary, in addition to such as is purely medicinal. Time will not admit of doing so, and I must trust to another opportunity of bringing them before my medical brethren, and meanwhile I will, for the purpose of facilitating discussion, briefly give you the conclusions which the observation of these cases has forced upon me.

1st. That we have arrived at a period when all are agreed that many uterine and ovarian affections are cured by homœopathic medicine alone, without escharotics. The

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catarrhs of the uterus, leucorrhœas, abrasive ulcers, and ovaritis, acute and chronic—the latter when it has become so serious as to have caused abortion again and again, and the cures have been confirmed not only by the ease of the patients, but by their going to the full period of pregnancy, when they had previously miscarried, it may be, several times, and where tonic measures had been employed by the allopaths without effect.

2nd. The diagnosis between uterine and ovarian disease must be carefully and accurately made. For though both uterus and ovary may be affected at the same time, and the symptoms consequently be complicated, we should miss our mark if we failed in tracing back the disease to its point of origin. The subjective symptoms are not unerring guides here in forming a correct opinion. They are often comparatively faintly marked when the lesion is serious—as in the case of large ulcers of the os and cervix, marked by nothing but weakness and yellowish leucorrhœa.

3rd. For diagnosis it is in many cases unnecessary to use the speculum, and it is never required where the ovary is clearly the seat of disease. In all cases, whether uterine or ovarian, I begin the treatment with medicine alone, and I commend this course to my younger brethren.

4th. When medicines carefully administered fail to benefit, and the symptoms point to the uterus as the seat of disease, then the speculum ought to be employed.

5th. (*Ethical remark.*) On the *first* occasion of using the speculum, and on all others, it is well that the patient should have a female friend or nurse present.

6th. When we examine and find ulceration of the os uteri of a granular character, and decide on the use of caustic, we should next select the kind of caustic. If there is strong reason for believing it to be of a syphilitic character, I should advise potassa fusa at once, and give a week for the separation of the eschar, and afterwards apply the *arg. nit.* every 4 or 5 days. Should the ulceration be of a common kind, begin with *arg. nit.* solid, and, as the healing process goes on, allow longer intervals between the applications.

7th. That escharotics ought not to be persevered with when they occasion *severe* pain. They are contra-indicated in such circumstances. Where they are really

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needed they give either *no* pain at all, or a mere transient burning, lasting for about an hour.

8th. That *belladonna* and *sepia* will remove the inflammatory symptoms which cause the pain, and pave the way for local applications if required. In such cases *carbolic acid* ought to be used instead of *arg. nit.*

9th. That abortions may often be arrested, even after hæmorrhage has taken place, by properly selected homœopathic medicines, and for this purpose *gels.* and *arnic.* are invaluable, not underrating, however, our old friends *bell., sepia, cocculus, sabina, secale, plat.*, and *ipec.*

10th. That ovaritis, if found to co-exist with pregnancy (as it sometimes does), ought to be treated persistently throughout the period, if need be; if not, the probability is that very troublesome inflammation, perhaps suppuration, may succeed the labour, which might have been prevented by forethought and appropriate treatment.

11th. That hæmorrhages from the uterus may in general be controlled by medicinal treatment alone, and that we possess a rich list of reliable medicines for that purpose, chief among which are *aconite, arnica, hamamelis, sabina, secale, crocus, platina, china, ipecac.*, and *millefolium;* but there are exceptional cases where the bleeding is caused by ulcers or polypus, and these may be suspected when hæmorrhage returns without any very obvious cause; and when such happens to be the case, examination should be made and surgical means be employed, in addition to the medicinal treatment indicated.

12th. That ulceration, if *thoroughly* cured, never returns; where it is said to do so, I believe it has never been *healed* entirely; and if a portion is left, it may be very easily ruptured, enlarged, and become as troublesome as the primary sore.

The most extensive ulcers may be cured without any injury to the constitution, and ovaritis, if it co-exist with ulcer, may be cured *after* the healing of the ulcer, and no transference of either disease be witnessed. On the contrary, healthy children being born, and the long life of the parents afterwards, prove the cases to have indeed been *cures.*

Canning Street, Liverpool, Sept. 1871.

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THIRTY YEARS' EXPERIENCE OF HOMŒOPATHY APPLIED TO SURGERY.

By G. Dunn, M.D. Edin.

Founder of, and Surgeon to, St. James's Hospital, Doncaster.

Mr. President and Gentlemen—

Nothing is better calculated to gladden the hearts of the pioneers of homœopathy than, after glancing back at the practice of medicine prevailing some fifty, forty, or even thirty years ago, to look at that which is taught in the schools to-day. Yet, while thanks to the labours of the immortal Hahnemann, and to the steady development of all branches of science, and more especially to that of physiology, the modern treatment of disease differs widely from that which shocked the founder of homœopathy, therapeutic methods still linger which are far from creditable to our art. Some years must elapse, I fear, ere the therapeutic law of *similars* is as generally recognised and as constantly put in practice as it admits of being. Signs there are, however, neither few nor indistinct, which prove to those who watch them narrowly, that a great change is stealing over the practice of the best of those physicians who still cling, partly from prejudice, partly from dread of losing professional caste, to the empirical practice which is understood when we speak of allopathy. We have only to compare Watson's *Practice of Physic*, published in 1843, with Reynolds's *System of Medicine*, or Tanner's *Practice of Medicine*, the chief text-books of the past and the present, to notice how great is the difference between the teaching of thirty years ago, and that which directs the medical practice in our time. Great as has been the gain which has accrued to the physician, I think that I shall be able to show that the surgeon has profited, if not to the same extent, yet very largely, by adopting the law of *similars* as his guide in the selection of medicines. While it is true that, to the merely superficial observer, surgery may appear more or less of a

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mechanical art, the success of the operator depends quite as much upon the accuracy of the diagnosis, on the preparation of his patient for the necessary operation; and on his treatment of him after its performance, as upon his mechanical skill in operating. Here there is oftentimes large room for medicinal treatment; and here it is that homœopathy compares so favourably with allopathy. Here it is that the surgeon who practises homœopathy finds himself possessed of measures for meeting the exigencies of practice so vastly superior to such as are at the disposal of his allopathic neighbour. Most truly can I assert that the wearying anxiety I once felt after operating, or when attending cases of severe injury, has been so much diminished as to have been almost removed since I became familiar with the safer, milder, and more certain remedial measures of homœopathy.

I must also here admit my obligations to the late Sir James Simpson's discovery of acupressure; and more especially to the antiseptic method of the talented Professor Lister, of Edinburgh. These two discoveries have added greatly in my opinion to the chances of success with which the surgeon undertakes the treatment of the wounds incident to operations, of those of compound fractures, and other grave injuries, as well as of abscesses and burns.

Time does not allow of my enlarging upon either method; but I would earnestly commend both, and more especially Professor Lister's plans of operating and dressing, as described in his *Address on Surgery*, recently delivered at a meeting of the British Medical Association at Plymouth, and published in the Association Journal of the 26th of August, 1871, to the careful attention of all who are interested in practical surgery. I am aware that in some of the London Hospitals Professor Lister's views have not met with much sympathy, St. George's, the Middlesex, and the London Hospital, are, I believe, the only metropolitan Hospitals in which the antiseptic method of operating and dressing is partially carried out; and, indeed, I saw only the other day, a colleague of Professor Lister's perform several capital operations in the Edinburgh Infirmary, in none of which did either acupressure or carbolic acid play any part whatever. The prejudice, or professional jealousy, which dogs the heels of Professor Lister's great surgical discovery is near akin to that which

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has followed Hahnemann's still greater—greater, because more far-reaching—reform in medicine.

Look for a moment at some of the results which follow the adoption of the antiseptic method. After a capital operation there is no necessity to dress the wound for a week. When, at the end of a few days, or a week, the stump is dressed, the unhealed surface is found as clean and free from all suppuration, as when the knife swept round it. This I have seen in several cases in my own practice since I adopted Professor Lister's plan; while in some complete union has, during this short space of time, taken place. Results so great as these, so important to the comfort of the patient and the success of the surgeon, cannot, I believe, be over-rated. I feel sure that in no long time the antiseptic method will obtain the sanction of all practical surgeons.

I now pass on to the more immediate subject of my paper, *the Influence of Homœopathy on Surgery*. In considering the advantages of homœopathy over empiricism, I select, for the purpose of comparison, the practice enjoined in the latest and most approved surgical text-books, viz., those of Professor Erichsen, of London, and of Professor Pirrie, of Aberdeen.

The superiority of homœopathy in surgical practice is rendered especially conspicuous:—1st, In diseases of the bones and joints, where amputation is, for lack of homœopathy, oftentimes resorted to. 2nd, In removing the consequences of traumatic shock. 3rd, In promoting the repair of injured structures. 4th, In so improving the general health of a patient, as to render successful an operation which, without this improvement, would probably have been fatal; and 5th, In hastening convalescence after an operation.

1. In diseases of the bones and joints, where, to save life, the limb is often sacrificed, homœopathy has achieved some of its most brilliant triumphs, and such medicines as *silica*, *calcarea*, and *sulphur*, have secured a place in our Materia Medica of the highest order. In illustration of this fact I will narrate briefly the particulars of two cases. The first, I believe, was in many respects similar to that of an illustrious Princess who had not, as you are aware, the benefit of our system. My patient was a young lady residing in the North Riding of Yorkshire, and was stated to be suffering from disease of the knee-joint. She

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had been under the care of the usual medical attendant of the family, and an eminent Leeds surgeon, now deceased. Her sufferings for three months had been great and constant; large and ever increasing doses of morphia and other anodynes had failed to secure sleep or rest of any kind. Closely enveloping the knee was a belladonna plaster; the joint being further rendered motionless by admirably adjusted splints and bandages. After removing these, and thoroughly cleansing the joint from its unpleasant surroundings, I found the knee firmly contracted, much swollen, very red, and intensely sensitive to the touch. The pain on lying down was so considerable, that my patient had abandoned all idea of going to bed. The pulse was 120, and her general condition indicated that irritative form of fever which so seriously complicates joint affections. The indications to be fulfilled were plain, sleep *must* be obtained, the pulse reduced, and the pain alleviated. It was bed-time when my examinations were completed; and I gave three globules of *aconite* of the 12th dilution in a little cold water. This simple and single dose, and the freedom of the joint from all artificial restraint, was followed by a refreshing sleep of eight hours, more sleep than the patient had had—morphia notwithstanding—for three months. The change seemed magical; and the entire removal of disease was thenceforth uninterrupted and speedy. The principal remedies were *aconite* in the first instance; and when pain and inflammation ceased, *rhus toxicodendron*, also in globules of the 12th and 30th dilutions. The knee being very much contracted, I advised the division of the hamstring tendons, in order to give more motion to the limb, this, however, has not, so far, been acceded to. Here is a case which, had it in the first instance come under the care of a homœopathic practitioner, whether physician or surgeon, would have been cured in a comparatively short time, while, as it was, the leechings, blisterings, plastering and drugging, which had been resorted to, had so aggravated and complicated its nature and injured the constitution of the patient, that her strength and patience were well-nigh worn out; and the knee permanently contracted. The young lady is, I need hardly say, a thorough convert to homœopathy; and has done, and I trust may long be spared to do, much medical good among the poor in her neighbourhood.

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Some six months ago this lady sent to St. James's Hospital a youth who was suffering from diseased knee-joint; and had been, for some months, an out-patient of the York County Hospital, where the treatment pursued was similar to that adopted in her own case. He came to my hospital on crutches, the affected limb bandaged from the foot upwards with very thick leather plasters; and was ordered to move the knee as little as possible; the leg was much smaller than its fellow. The nature of the case was somewhat different. He was of a strumous habit or constitution; and the ends of the bones were enlarged with scrofulous inflammation of a low type and atonic character. I stripped the joint and limb of its plasters and bandages, gave him *calcarea carbonica* in globules of the 30th dilution, night and morning; and a good, nourishing diet. In a few days after his admission I was able to send him home; and he very soon resumed his duties in the Post Office, in which he was employed, and his recovery was rapid and complete.

The allopathic treatment of such cases—as now taught in the schools—is only one degree milder than it was in the early days of the late Sir B. Brodie, who, as you know, in the earliest editions of his work *On Diseases of the Joints*, strongly urges the use of the actual cautery, moxas, leeches, and blisters and mercurial plasters. Some years, however, before his death, becoming convinced of the injurious effects of such practices, Sir Benjamin publicly and honestly denounced them as fraught with evil to the patient. Professor Erichsen, in his work, recommends leeches and evaporating lotions; Professor Pirrie, leeches, cupping, and evaporating lotions, with the "antiphlogistic regimen," which I take to mean semi-starvation. Such is the treatment which passes as "orthodox," while to cure synovitis with *aconite* and *rhus*, and scrofulous enlargement of bones with *calcarea*, renders a surgeon liable to be dubbed a quack. To enlarge upon the power of *calcarea* in controlling diseases of the osseous system, especially in strumous constitutions, is, I think, unnecessary here.

Gangrene, so terrible in its aspect, is another disease in which I have seen much advantage from the use of its truly homœopathic specific, *secale cornutum*. Many cases have I seen cured where the best allopathic treatment could not have averted either amputation or sloughing.

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Erichsen, in writing on gangrene, cautions surgeons not to make too free a use of antiphlogistic remedies; but yet he favours local blood-letting. Pirrie says: "It should "be kept in view that it is in persons of debilitated "powers of constitution that gangrene very frequently "occurs; and that in them it is only the most cautious "use of antiphlogistics that can be borne even at the "earliest periods, for the powers of the system may thus "be so lowered as to make them fall victims to gangrene "in even a comparatively slight inflammatory attack;" but he goes on to say: "In an individual with a robust constitution, the early and judicious employment of blood-"letting is necessary." But in a following paragraph I quote with pleasure these sensible words: "Every year's "experience impresses me more and more with the im-"portance of guarding against impairing the general "powers; and in no class of cases is this caution more "necessary for the subsequent safety of the patient, than "in the treatment of many inflammatory affections." Then come some judicious remarks on the general treatment, but no word of a specific remedy. Professor Lister, in the address I have already alluded to, narrates the case of a patient with gangrene of the little toe, which I am morally certain would have been cured by any homœopath with *secale cornutum*. The Professor amputated the toe, and so far was that from a cure, that he says, after a year's respite, the patient's proclivity to the disease had been shown, a few days previously, by its appearance in the tips of all the remaining toes of the same foot, though the scar of the former operation remained sound. Would that such an ornament to his profession could be induced to try the *ergot* in such cases.

The great success of homœopathic treatment in preventing the necessity of amputation, and many minor operations, has been also abundantly proved during my thirty years' practice, throughout eighteen of which the St. James's Hospital has been open to all comers; but the time at my disposal will not enable me to single out cases. I cannot, however, refrain from mentioning the great value of the *hydrastis canadensis* in chronic ulcerations; and more particularly in those of a cancerous nature. Judiciously used, I dare not say that it will cure; but the experience afforded by the treatment of many cases, assures me that it will relieve and hold in check this very

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formidable disease more thoroughly than any other remedy I have met with.

2. The consequences of a shock sustained by severe injury are more rapidly removed, more completely removed, and more safely removed, by homœopathically indicated remedies than by any other known form of treatment. Cases of sudden injury from blows and falls on the head, are frequently brought to St. James's Hospital in a state of unconsciousness. Some years ago a boy fell from the bridge, over the railway, a height of 36 feet. He was brought to the hospital unconscious. His treatment consisted of rest in bed, and the administration of *arnica* in globules of the 12th dilution dry on the tongue; and a weak solution of the same drug applied to the head. The effects of the concussion continued for three days, when he awoke as from a long, sound sleep, and asked, "Where am I?" In ten days he was able to leave the hospital perfectly well; and has remained free from all traces of injury from that time to the present. Local or general blood-letting is almost invariably recommended by surgeons in such cases; but I am persuaded that in them where there is fever, we have an infinitely superior agent in *aconite;* and where there is little or no fever, *arnica* renders essential service.

3. In promoting the repair of injuries such as fractures and contusions, the surgeon who practises homœopathy has an immense advantage over his allopathic colleague in the knowledge he possesses of the remedial power of such medicines as *arnica, rhus, calendula,* and *symphytum.*

My own case will serve as an illustration of this proposition; and as I had the almost constant attendance of one or more of my allopathic neighbours, I can compare the practice pursued by myself with that which, in all kindness and conscientiousness, was urged upon me by them. I was also visited in my distress by some of my homœopathic brethren, one of whom I am pleased to see present with us to-day; and who will, I hope, correct me if I exaggerate. In February, 1865, I had a severe fall from my horse, resulting in fractures of the ischium, ileum, and the clavicle of the left side, together with sundry flesh wounds and contusions in divers parts of my body, more especially in the face and over the left orbit. After a most painful cab-drive, I was carried to bed, my nearest allopathic neighbour sent for; and a telegraphic message

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summoned my nearest homœopathic brother. I suffered much agony, as I suppose all do suffer who have two or three broken bones with a few cuts and contusions thrown in, as it were, to make the account complete. But I was fortunate in being spared my reason; and in being enabled to weigh the measures that were used, and those that were proposed for my restoration. I believe that my recovery was regarded as almost, if not entirely, hopeless by my allopathic friends; and that the best issue that could be expected was life, the use of my legs once more being deemed an impossibility. I was placed in a position as comfortable, I suppose, as circumstances would admit of, at the best, however, pain was severe and constant, it being impossible to breathe without some disturbance of the broken bones, while to cough was agony itself. I at once took globules of *arnica;* and my homœopathic friend here dressed my lacerated wounds with an infusion of *calendula.* In the evening my allopathic attendants sent me a composing draught of Liquor Opii, this I directed to be put—not into my stomach, but—into a drawer. For several days and nights sleep was a stranger to me. As soon as it could be done with any degree of safety, I was placed on a water bed; and the comfort I derived from it was far more than words can express. At last I got four hours continuous sleep, and rapidly then regained health and strength. My recovery was not partial, but complete; and now, after the lapse of six years from the date of the accident, I am ready to run a race with any man of my own age!

I should not have cited my own case, but that I think it shows, in a very striking light, the advantage of adhering strictly, in the severest and apparently most hopeless cases of injury, to the use of such medicines, and such only, as are selected on the principle of "*similars.*" Many and severe were the contests I had with my allopathic friends, who were never-ceasing in the urgency with which they pressed upon me the necessity of taking this or that drug. Opiates, purgatives, fever mixtures, *et id genus omne,* were held up to me in endless variety, but all to no purpose. Fully persuaded that such injuries as I had sustained could only be repaired by keeping the body in a healthy state, being equally persuaded that such a state could not be preserved while taking such indigestible materials, I relieved my sufferings by medicines

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homœopathic to them. The results astonished—though they did not convert—my allopathic friends. I must excuse them, however, on the ground of age, they being all over forty. The medicines which gave me most relief were *arnica, aconite, bryonia,* and *calcarea.* I always took globules of the 30th dilution if I had them, if not, the next to the 30th.

I have often been surprised at the rapidity with which fractures, both simple and compound, recover when the attendant disorders are treated homœopathically. As a further example I may give the following case, which came under my care at St. James's Hospital. A brewer's waggoner was sitting in a drunken sleep on the shaft of his waggon, when he fell, and both fore and hind wheel went over him, the former causing fracture and severe contusion of the right thigh, the latter breaking the tibia and fibula of the left leg. There was also a scalp wound, from which considerable hæmorrhage took place, and other minor injuries. The man, though intoxicated at the time, was not a confirmed drunkard. The fractures were reduced in the usual way, and the febrile and other symptoms met by homœopathically-chosen medicines. Notwithstanding the severity of the injuries he had received, and the fact of his being one of a notoriously unpromising class of persons—brewer's carmen—to recover from severe injury at all, he walked with the aid of crutches out of the hospital at the end of six weeks. In short, the influence of homœopathy in assisting in the cure of injuries has led some of my allopathic colleagues to remark that accidents treated in St. James's Hospital recover more speedily than those that come under their care. I don't mean to say that they acknowledge that homœopathy has had anything to do with it; but were it not for homœopathy, I feel sure that they would not have been able to arrive at such a conclusion.

4. I have been much impressed with the advantages of homœopathy in improving the general health of patients whose cure required an operation, but whose exhaustion was so great as to render it unjustifiable. This I have especially observed in cases of long-standing necrosis. In several of these, when brought under my notice, the long-continued drain upon the system had so exhausted the vitality of the sufferer, that any attempt at mechanical interference would probably have been fatal.

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By the use chiefly of *calcarea*, *silica*, and *sulphur*, I have been enabled so far to improve the general tone and powers of nutrition as to admit of the usual operation for the removal of a sequestrum being performed with safety and success.

5. In hastening convalescence after an operation I owe much to homœopathy. The use of chloroform, no doubt, tends to reduce the shock of an operation; but chloroform notwithstanding, the surgeon must always be on his guard against febrile excitement. In meeting this condition the homœopathist has a double advantage. *First* of all, he need give no opiates; and opiates, I feel sure, have much to answer for in the production of the febrile reaction which occurs after amputation and other major operations. *Arnica* does all that *opium* can do, and does not do that which *opium* does, but which the allopathic surgeon wishes much that it would not do. *Secondly*, by enabling us speedily, and without any injury, to meet conditions which tend to impair nutrition, the process of healing is more rapidly gone through, while at the same time, where Lister's plan is not adopted, such a remedy as *calendula* still further promotes it.

The time allotted to me in which to address you must have been exceeded, and I must therefore draw my remarks to a close; but I think I have said enough to show that, in selecting his medicines upon the principle of *similars*, the surgeon has, in considerable and very important respects, an incontestable advantage over one who has but the uncertainties, and, I would add, the dangerous uncertainties, of "old physic," to fall back upon.

I had intended, in accordance with the original title of my paper, to make a few remarks on the advantages of homœopathy in obstetric practice. To this very important branch of my subject I feel that I cannot now do any justice at all. I will, therefore, content myself with observing that during a practice, involving attendance on 2,400 cases of labour, I have repeatedly had occasion to feel grateful to the great discoverer of that mild and beneficial system of medicine of which we here assembled are the advocates.

Doncaster, September 1871.

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THE RELATION OF HOMŒOPATHY TO SURGERY.

By Wynne Thomas, M.D. Lond.,

Surgeon to the Birmingham Homœopathic Hospital.

In a recent Report of the Birmingham Homœopathic Hospital, it was stated that several important operations had been performed during the past year. An allopathic medical journal, alluding to that statement, said it could not see how homœopathy could help to cut a man's leg off. Now if there were never any necessity for the administration of medicine before or after the operation, this would have been a very proper remark; but as the writer well knew, surgery and medicine cannot be thus divorced.

It is the province of medicine to *intercept* the knife, to precede or follow it; in other words, to cure without operation, to prepare the system for undergoing an operation, or to carry the patient safely through it. And homœopathy is able to accomplish all this. Many a case is cured by homœopathy that, without it, would have required the knife; and all operations may be, and are, rendered more successful by its assistance. According to the nature of the disease, so is the *surgical* or *medical* treatment the more *important.*

In the treatment of stone in the bladder, for instance, at present an operation is the first in importance, and medicines can only step in to help in healing the wound and in restoring the power of the constitution. In other cases surgical manipulation is only the accessory, as in retention of urine from cold, where the catheter, however absolutely essential it may be as a temporary aid, is only resorted to as an expedient for gaining time until medicine has restored the natural function.

I wish in the first place to speak of the use of local applications; for I conceive that these belong to surgery, equally with the use of instruments or mechanical appli-

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ances. Now I believe that, rightly or wrongly, an objection is felt by most homœopaths to the use of local medication, because of an apprehension that the constitution may be injured thereby. But in my own experience I have found it of the greatest service, and, in truth, I hardly know how it would be possible to do without it.

It may be laid down as a maxim, that the more local a disease is, the more difficult it is to treat it by internal medicines. The very paucity of the symptoms constitutes often an insurmountable obstacle to selecting the proper medicine. Take, for instance, an ordinary hydrocele. I suppose all who have had to do with it have given up the task of discovering an efficient medicine in despair. Between this case, which may be regarded as at the extreme, and other diseases—such as eczema—which are, only in a much inferior sense, local, there is every variety to be found; and not only is there a difficulty in treating local disease by internal medicine, but it is true that, almost in proportion as a disease is local, so is it quickly, safely, and easily cured by local means. It seems to me, then, wise, if this be the case, to acknowledge the fact, and see whether the law of *similars* cannot—as I believe it can—be extended to the selection of the local medicament.

It must be conceded that mischief has followed the attempt to cure disease by local means, and I apprehend that this has happened from not bearing in mind the difference between cases which are primarily local and those which are secondarily so. In considering this matter I shall, therefore, divide local cases into two categories:

1. Those which begin at a certain spot, and either confine themselves to that place or afterwards affect the constitution;
2. Those which begin as constitutional affections, and exhibit local manifestations afterwards.

The first class of cases can be safely treated locally; and I know not why, if they can be so treated, we should wish, even if we could, to reach them by the more circuitous route of the general circulation.

The difficulty will often be to say to which division a case belongs. I do not object, of course, to using internal medicines as helps; I am only now arguing against those who object to local means.

I should place in the first division—Local injuries,

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sprains, inflamed bursæ, varicose ulcers, bunions, ingrowing toe-nail, corns, warts, tumours, cysts, hydrocele, epithelial and other cancers, polypi, parasitic skin diseases, catarrhal ophthalmia, gonorrhœa, simple chancre, chilblains, &c.

A large and widening field for investigation has of late years been cultivated with results which lead us to think that parasites have a more considerable share in the production of disease than has hitherto been admitted; and the more this can be shown to be the case, the more diseases will be placed in our first division, and consequently the more shall we use local medication. I have been much surprised, since I have had the care of the inmates of an orphanage, to find that several diseases occur epidemically; at least, we rarely have isolated cases; but certain ailments nearly always come in crops. There are enlarged glands, of which we have two crops a year regularly; gathered fingers, styes, stomatitis. I am not prepared to say that these have any connection with parasites, but I have thought the facts worth stating.

Of the known parasitic diseases, herpes circinnatus is the most common. Of this we have two large crops annually. Some time ago the matron assured me that every child and servant had had it. We find it very easily curable. *Carbolic acid* and *glycerine* is our favourite application. *Sepia* has been regarded as the specific for it; but, after a good long trial, I have given it up as powerless either to prevent or cure. It is worth noting that when the herpes first got into the orphanage, the cases were much more severe. It seems as though the seed had lost some of its vitality.

The second class of local diseases are the outward and local manifestations of constitutional affections, and are clearly secondary in importance to them. Good examples would be gout, rheumatism, eczema, piles, &c. I think we should all feel it wrong, even if it were possible, to cure these complaints by local means only. We regard them as in some way or other an attempt of nature to evacuate some poison, or at any rate—without accepting the humoral theory—we consider that, if these outward ailments were cured, some viscus would probably suffer vicariously. This entirely agrees with my own view; but I think that even here there is room for local treatment. I would rather, if possible, cure with medicine.

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I should always endeavour to do so; and even when I resorted to a local remedy, should at the same time *continue* the constitutional treatment.

The majority of practical men, allopathic and homœopathic, agree, I think, that in such a disease as eczema there often arises a time when, the system being relieved, the skin seems to have lost its recuperative power, and that then a local stimulus greatly promotes the cure.

I would illustrate this by two cases.

A girl of 13 had a patch of dry eczema on the nape of neck. I failed, after four months internal treatment, to make any impression on it; the girl's health was good. I then chose a homœopathic application, viz., *tinct. lyttæ*, and applied it freely over the patch. It produced an artificial eczema—in fact, blistered it, and in fourteen days the skin was quite healthy, and has remained so.

A lady, of 60 odd, had long suffered from eczema behind the ears. I treated her constitutionally, and, not succeeding, placed her under one of our best physicians in London. He also failed, after a two months' trial, and she again returned to me, asking what was to be done, she suffered so much from it. *Oil of cade soap* in about a week nearly cured her, and she has not suffered since. In neither case have I observed the slightest symptoms of ill health following the application. I must acknowledge that the last was used empirically, but I believe a proving would show that it could produce an eczema.

What would you do with a phagadenic ulcer, I ask, if you must abstain from local treatment? I was attending a gentleman with a large serpiginous ulcer of the leg. In spite of all my internal medicines, selected with the greatest care, the ulcer spread at one edge as it healed at the other. Being under the impression that I ought to cure it by medicine alone, I persevered, and lost much valuable time, and nearly lost a valuable patient. I at last resorted to the old application—*nitric acid*. I touched all the sloughing edges with it, and had the pleasure of seeing a healthy action immediately commence.

I know not how we could arrest sloughing ulcers of the throat without it. Now, it appears to me that if we would only interpret the law of *similars* more freely, we should find it of the greatest assistance in selecting local applications. I know that there are not wanting men who do this, and meet with great success. Perhaps there

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is no substance so useful as a local application as *nitrate of silver*. We have learnt its use from our allopathic brethren; but I think we may explain its action on homœopathic principles, although I confess, as in most local remedies, the resemblance between its action and the disease is only a rough one; but at any rate, I have not found anything to supercede it in the following cases: ulceration, stomatitis, catarrhal ophthalmia, irritable or indolent ulcer, violent exhaustive cough from uvular irritation, &c.

It is not necessary for me to illustrate the action of remedies such as *arnica* and *calendula;* these are too well known and too much valued to need it.

Sulphur, as an ointment, is not only required in itch, but also in some forms of eczema. Probably the best application in chilblains is turpentine, and in burns the old oil and lime-water holds its ground.

I will now try to give you my estimate of the value of our remedies in the internal treatment of surgical cases, and will first speak of inflammation, which meets us in some form in almost all cases. It will be granted that whoever can best treat inflammation will be the most successful surgeon. Now, I think that no medicines and treatment can compare with ours. I should be extremely sorry to ever have to treat inflammations without *acon.*, *bell.*, and *merc.* It is extremely difficult to particularise much, because, as we all know, remedies have to be chosen, not merely in accordance with the symptoms of the disease, but also with the temperament and constitution of the patient. For instance, an ophthalmia which in one patient will require *merc.*, will in another need *calc. carb.*

Now it has been my experience that *aconite* is remarkably useful in averting inflammation after operations. In other cases *bell.* and *merc.* have done excellent service. *Veratrum virid.* and *rhus*, too, have been of use. When the suppurative stage has been reached, *hepar*, *silica* and *phosph.* are eminently good. Abscesses are more quickly ripened. Fistula, as in the breast, I have seen rapidly heal under *phosph.;* and I can think of one long standing fistula in ano which perfectly healed up under *phosph.*, *calc.* and *silicea.* In these latter cases we shall frequently be driven to an operation, and, if the internal opening be free, I am afraid it is, so far as our present knowledge

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goes, inevitable; but even if the operation be necessary, there is much to be done by medicine in improving the health of the patient and the healing of the wound, which I believe does not so generally heal up perfectly as writers lead us to expect.

When the inflammation is of an erysipelatous kind, *bell.* and *rhus* and *apis* are of undoubted value. I have cured many severe cases with *acon.* and *bell.*

A still worse form of inflammation is the carbuncular. In one case I succeeded in a few days in dispelling the hardness by *silica;* but in my experience this is exceptional, and we must expect to see the process of suppuration completed. All the cases that I have had to treat of this kind, of late years, have been soon cured; but I have not had to do with a very severe form, and I think I should probably again have recourse to the crucial incision to arrest it, if the disease continued to spread. In these cases, medicines such as *silica, hepar, ars., lachesis* and *carbo veg.* are of much service. I have found *ars.* act admirably in some cases of sloughing; in others I have failed to avert it without a local application of nitric acid. I am sure that we can much oftener dispense with the knife in the various results of inflammation. I rarely open an abscess, but allow it to break; knowing that medicines will sufficiently hasten the discharge of pus.

I will now enumerate some kinds of cases in which we can occasionally avoid operating.

I have cured one case of ranula with *merc. viv.*, and have seen polypus of the nose several times so much reduced as to cease to be troublesome, under *calc. carb.* or *kali bichrom.* Varicose veins can often be reduced by *hamamelis* or *puls.*, and nævi will occasionally diminish considerably under *arsenic.* In only one case of hydrocele has medicine proved sufficient to prevent the re-accumulation of the fluid, after tapping without injection. The case was in an elderly gentleman, who had psoriasis at the same time. It is worth mentioning that once I succeeded in returning a femoral hernia, after a course of *nux,* lasting over several weeks, which resisted my repeated efforts before the administration of the medicine.

I have seen one cataractous lens recover its transparency, and in several cases have seen a decided improvement produced by *sulph.* Piles can often be cured,

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and generally so diminished as to render excision unnecessary. Fissure of the anus, which has generally to be treated by the knife, will sometimes heal under medicines, as *graphites*. *Lachesis* approves itself in prolapsus ani. I have seldom seen any effect produced on other tumours, such as fatty, cancerous, fibrous, or cystic; but lately a case of Mr. Lawrence's, of undoubted cancer of orbit, has wonderfully improved under *carbo anim.* 200. This case was pronounced to be cancer by the best surgeons in our town. The eyeball was pushed much out of place, and the pain was so severe as rarely to allow the patient any sleep. He took our powder of *carbo anim.* 200 once a week, and sugar of milk on the other days. The man voluntarily remarked that the first powder every week had a strong effect. When I saw this man, the eyeball had returned to its central position, and the pain was trifling, but there was still a considerable swelling on the side of the nose.

There are scattered through the pages of our periodicals cases of different kinds of tumours which have disappeared under treatment; but as I wish to confine myself to what has passed under my own observation, I do not dwell upon them. I have no brilliant cases of the kind to relate. There is sufficient evidence to make us hope that some day more may be done in such cases.

Two striking cures of corns have obtained for me considerable repute. I think they are worth relating. A gentleman had been under the hands of a corn-cutter for five months, who often pared down the corns which were on the sole of the foot, but in spite of all he could do, the corns multiplied, till at length the patient was so lame that he advised him to consult his surgeon. When he came to me there were five corns ranged across the front of the sole. I was so sceptical as to the power of medicine, that I hesitated to prescribe; but he being a more steadfast homœopath than myself, I assented to his request, and gave him *thuja* 6 and *natrum mur.* 6. Nothing was done to the corns, nor did he rest, for he had to take a journey to London. In six weeks he called and showed me his foot, which was perfectly clear.

About a year afterwards, another gentleman consulted me about similar corns on the sole of the foot. I told him of the former case, and asked him to let me treat him in the same way, without any local application. He returned

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to his house in Ireland, and in five or six weeks wrote to say that during the three weeks that he had been taking the first medicine not the slightest change was perceptible, but that the day after beginning the second medicine the pain suddenly ceased, and the corns soon afterwards came out.

I must observe that these corns were of a peculiar kind. I only recollect having seen one other case similar. The corns were not so hard as those on the tops of toes generally are, and, when looked at closely, were seen to have a number of fine papillæ, exceedingly sensitive to the touch, in appearance a good deal like the papillæ filiformes of the tongue as viewed under the microscope. I have referred to all the surgical works within reach, and have not seen them described. I am not sure that they ought not to be called *warts* rather than corns. I have tried the same medicines in a case of hard common corns without success, so I think it probable they are only suited to that particular kind of growth.

These are remarkable illustrations of what internal treatment may do for diseases apparently local.

In no class of cases is our success more signal than in bone cases. I frequently see cases at the hospital, which have been long under other treatment, rapidly improve under such medicines as *calc.*, *phosph.*, *silica*, and *nitric acid;* and I could relate several in which I saved limbs that had been ordered for amputation by eminent surgeons. It is of course easier to estimate the value of medicines in caries than necrosis, because in the latter affection the duration of the process of separation depends much upon whether the sequestrum is on the outside or inside the living bone.

I find the after treatment of operations as a rule simple and successful. I generally administer *aconite* and *bell.* as soon as the reaction has begun. In this way inflammation is frequently avoided and union much facilitated. In dressing the wounds we use either simple water or *calendula; silica* is useful if the granulations are flabby, and *phosph.* when suppuration is copious. I unfortunately lost one case of pyæmia, and have failed in ovariotomy from peritonitis. Of all cases, those implicating the peritoneum are the worst; and I am sorry to say I have not yet found a medicine capable of subduing peritonitis when extensive.

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I will only add a few words upon accessories: and first, opiates. How far can we do without them in surgical cases? I find that I very seldom give *opium* in large doses; but there are certain cases in which, I suppose, we could not do well without it; as, for example, cases in which it is necessary to produce a quiescent state of some organ, as when the bowels have to be confined after stitching a lacerated perineum—or when a catheter has to be kept in the bladder, the organs must be rendered insensible to it. It will happen, too, that pain is killing a person, and the one thing necessary for him is sleep. In these cases we cannot do always with *cham.*, *coffea*, &c.; but in my experience these cases have been few. *Acon.* and *bell.* have usually subdued the excitability sufficiently to permit sleep; and in some cases where, from the severity of the operation, I thought an opiate necessary, I have repented giving it, because of the disturbance it caused to the general treatment.

I conclude by remarking, that while I use local applications more freely than I once did, my confidence in general homœopathic treatment increases with my knowledge of it; and that I may not appear to express this opinion without having had a fair opportunity for forming it, I may be permitted to say that my experience has been considerable, having performed in the Birmingham Homœopathic Hospital 72 operations, including ovariotomy, lithotomy, amputations, extraction of cataract, &c., while, from having filled the posts of resident surgeon and honorary surgeon in large allopathic hospitals, I have the advantage of being able to compare the results of the two kinds of treatment.

Paradise Street, Birmingham, September, 1871.

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TRANSACTIONS

OF THE

BRITISH HOMŒOPATHIC

CONGRESS,

HELD AT YORK,

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PREFACE.

THE Annual Congress of British members of the medical profession practising homœopathy was held at York, on 4th September. The following papers were read, abstracts of which, together with the discussions upon them, appeared in the English, American, French, and German periodicals.

Each member of the Congress is entitled to a copy of the Transactions; a list of members present, and the Officers of this and previous Congresses, together with those appointed for the next meeting, are given below.

The visitors and delegates from America and France joined in the discussions, and gave the members the benefit of Transatlantic and Continental opinion and experience on the subjects treated of in the papers.

It was decided to hold the next Congress at Leamington on the second Thursday in September, and Dr. SHARP was elected President.

The Executive Committee were requested to arrange the business for the next Congress, and to invite duly-qualified Veterinary Surgeons to future meetings.

(Signed) J. GIBBS BLAKE, M.D. Lond., Birmingham;
J. H. NANKIVELL, M.R.C.S., York.
} *Hon. Secs.*

November 1872.

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1870—BIRMINGHAM.

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1871—OXFORD.

President—Dr. Madden.

1872—YORK.

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Visitors.

Dr. L. E. Ober, Lacrosse, Wisconsin, U.S.; Delegate from the American Institute of Homœopathy.

Dr. C. A. Bacon, Surgeon to the New York Ophthalmic Hospital, New York, U.S.

Dr. Pilling, Platteville, Wisconsin, U.S.

Dr. Vincent Leon-Simon, Paris.

1873—The Congress is appointed to be held at Leamington, September 11th.

President—W. Sharp, M.D., F.R.S.

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Executive Committee.

Dr. Drysdale, Dr. Black, Dr. Sharp, Dr. Gibbs Blake, E. Fraser, Esq., Dr. Collins, and Dr. Wynne Thomas.

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ATTITUDE OF THE MEMBERS OF THE MEDICAL PROFESSION TOWARDS SPECIFIC MEDICINE.

By F. Black, M.D.

It is now 82 years since a methodical attempt was made to reduce the therapeutic art to a definite rule. Six years of farther careful experiment, and this rule was given to the profession as the clue to specific treatment. This announcement was made, in 1796, by Hahnemann in a remarkably able paper which appeared in *Hufeland's Journal.*

The rule he gave was that medicines tend to cure diseases similar to those they produce. He shewed the uncertainty of therapeutics founded *ab usu in morbis* when no law existed to unite the experiences into definite order. He felt that some successful hypothesis must be made which shall express the relation between the curative and some other discoverable property of a given drug, and that relation he believed to be one of similarity. This essay was soon followed by others which, while they advocated the value of direct and specific treatment of disease were from their very nature opposed to the prevalent medical opinions, and practices of the day. All reforms involve some destruction; each reformer is an innovator, but it was the merit of Hahnemann that he reverenced the temple though he could not worship the idols it contained. Against these he made unflinching war at a time when his voice was the solitary protest against the dangers of depletion by lancet, and purging, against the rash and imprudent use of powerful drugs whose physiological action was unknown, and where the prescriber's skill was exercised not so much in selecting the remedy as in stringing together as many as possible in one prescription.

He condemned polypharmacy, and advocated the em-

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ployment of single remedies whose action on the healthy body was known, and he pointed out that pathology alone is an insufficient guide in the treatment of disease, and that a law of cure must be independant of any theory of disease.

Soon a storm of opposition assailed him, and this was increased when in after works he advocated the employment of minute doses. For long he spoke of his system as the Doctrine of Specifics, and he employed the term homœopathic and allopathic as denoting certain therapeutic relations.

Time has fully justified Hahnemann's foresight as a reformer, for now in every country where medicine is followed as a science, the evils he shewed have been fully admitted and vigorously denounced, though his therapeutic teaching is still rejected by a very large majority of the profession. Hahnemann and his school have met with an amount of rancour, calumny, and opposition unheard of in medicine, even though it has to blush when it recalls the reception it gave to the discovery of Harvey, and the abuse it heaped on Jenner. "It was," wrote a distinguished enemy of cow-pox inoculation, "the damnedest thing ever proposed; he wished the inventors were all hanged, and he would give his vote for its being done.*

That Hahnemann's views must meet with some of the opposition they have encountered was to be anticipated. Sir James Simpson keenly feeling the opposition to his introduction of etherisation and chloroform writes:—

"From time to time in the march of medicine and other allied sciences, some earnest and expanded mind conceives and elaborates a great and novel thought, destined in its practical application to ameliorate and promote the happiness of mankind. But hitherto almost as often as the human intellect has been thus permitted to obtain a new light, or strike out a new discovery, human prejudices and passions have instantly sprung up to deny its truth or doubt its utility; and thus its first advances are never welcomed as the approach of a friend to humanity and science but contested and battled as if it were the attack of an enemy. Practical medicine, in its past career, is full of instances illustrative of this remark."

Let us take one instance of a great physician who was abused when alive, and is now worshipped as the modern

* Moor's *Reply to the Anti-Vaccinists*, 1806, p. 14.

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idol of medicine. "The great Sydenham," says Dr. Lettsom, "for all his labours only gained the sad and unjust recompense of calumny and injury," and his brethren of the Royal College of Physicians endeavoured to banish him as guilty of medicinal heresy.

You are all familiar with the reception Hahnemann's views of Specific Treatment have met with: a sower of seed may be pelted with mud, distressed and disturbed in his work, still the seed sown must germinate, and in spite of all opposition the employment of specifics according to a defined general law is adopted by thousands of medical men all over the world.

Let us briefly turn to the reception Specific Medicine has met with in England. About 1830 Dr. Quin commenced in London to advocate these therapeutic views, and in 1832 had acquired a reputation sufficient to attract the attention of the Royal College of Physicians, who by summons admonished him to desist from practice till they had duly examined, &c.

It was prophesied by one of the College Censors, a physician of great eminence, *that as homœopathy could not exist two years*, it was unnecessary to take steps against Dr. Quin. Whether this belief was general or not, no steps were taken, no penalties were enforced. What, gentlemen, do you think of the prophecy? here we meet to bear testimony to the value of that specific law of healing which forty years ago the Censor destined to live two years. How often the wish has been father to the thought, and equally premature forecasts of our death made? Yet Saturn, that devourer of shams and lies, though he has swallowed up many a medical theory and practice since 1790, has spared us. If steady vigorous growth implies vitality, we have all reason to take heart, and to congratulate mankind on the existence of a law of healing which we sincerely believe is well calculated to place therapeutics on a sure basis.

History is ever repeating its recitals of new truths meeting with more or less opposition, with more or less suffering to the advocates, but such obstacles though retarding never extinguish the truth.

Mankind never seems to learn the lesson that persecution is an unavailing weapon, that the truth of a doctrine must be proved or disproved by other arguments. As no amount of minting will coin falsehood into current truth,

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so no mere pressure of brute force will crush life out of truth, and the repressive form of argument employed by the medical bodies is simply an application of one of the worst forms of physical force, and has no pretentions to logical argument. Seeing then that it is so general a law that new truths must meet with opposition, the inference is that this must have some beneficial influence on those who uphold the truth. Let us examine into this point, first characterising the nature of the opposition, and then its bearing on the advance of therapeutics, then its influence on us its advocates.

We exclude from this consideration the opposition which each of us occasionally meets in his local sphere of practice, characterised by gossiping tittle tattle varying from the petty to the diabolical, such attacks ought never to be publicly noticed, but manfully outlived.

The opposition we desire to consider is that which is passive and that which is active.

The passive starts on the assumption that the doctrine of specifics is a dangerous fallacy, but to attack it openly were merely to fan the heretical flame, and give it unmerited notice. A conspiracy of silence is therefore determined on, the name is never to be mentioned in so-called orthodox circles, and rigidly excluded from all medical publications—nay, the very advertising sheets of such periodicals are regarded as too pure to be tainted by the advertisement of any works treating on the subject, and still worse, works independent of therapeutics, but whose authors practise homœopathically are denied circulation among medical booksellers. Such a policy is pursued in the vain hope that the hated heresy must thereby die of inanition.

Now, like the false prophecy of the Censor of the Royal College of Physicians, history is shewing the unsuccess of this policy, and the bad effect it has on those who pursue it.

A duration of eighty years, a steady growth in spite of an opposition unparalled in the history of medicine may not be sufficient to establish the truth of the doctrine, but it is far too great to allow it to be taken for granted that Specific Medicine is a fallacy, and must therefore die if never adverted to.

The active opposition starts on the determination not only that homœopathy has not been proved a truth, but that it shall not be proved such.

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It is this they say, so utterly opposed to science and common sense, as well as so completely at variance with the experience of the medical profession, that it ought to be in no way or degree practised or countenanced by any regularly educated medical practitioner.*

These reasons are brought forward by men who have never practically tested or even carefully studied the Doctrine of Specifics. They are mere excuses to give a shadow of right to a tyranical use of one of the worst phases of trade unionism, to anathematise not only those who practise a system of therapeutics of which they are ignorant, but to extend the excommunication to any who dare to meet the heretics in consultation.

If anathemas proceeding from such associations were laid for decision before a jury of scientific, not medical men, what would be their verdict? Would they not answer, such a course is entirely out of place in science, and still more inapplicable to the practice of medicine? They would adjudge that as an example it was destructive to all progress, and an insult to science, for it attempts to fetter thought, but will and truth cannot be denied the everlasting right of free growth.

Such decisions could only be consistent in men who possess a creed, and what creed do associated medical bodies hold? None—but without a creed there can be no orthodoxy and no heterodoxy.

If the general belief of the profession can be moulded into a creed, it must be negative, and might be worded as follows: First, whosoever will be a member of our body, before all things it is necessary that he hold the homœopathic faith in utter abhorrence.

Second, we hold this abhorrence all the more binding, though now so many eminent men of our own school have expressed their belief in the poverty of our own therapeutics, and that "the greatest gap in the science of medicine is to be found in the stage of therapeutics."†

These resolutions were not only unanimously adopted by the three hundred medical men present, but these Brighton resolutions have become the law and practice of the profession; not a voice protested, though not a fortnight before the Brighton meeting Sir R. Christison in

* Resolutions of Provincial Medical and Surgical Association. Brighton, 1851.

† Sir J. Watson's Address to the Clinical Society, 1868.

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his Inaugural Address to the Edinburgh graduates said, "Of all the medical sciences therapeutics is in the most unsettled and unsatisfactory state, and the least advanced in its progress."

Even now a Journal whose *raison d'être* is the poverty of therapeutics, and which in its preface states "the science of healing has remained very nearly where it was when Rousseau exclaimed, '*Laissez-moi mourir, mais ne me tuez pas,*'" dares not to admit an article written by anyone who believes that the homœopathic law professes to fill up the gap in therapeutics.

We believe that there are not a few eminent men who disapprove of this mode of opposition, but they are overawed by the cry of the professional crowd—down with it, down with it—and thus in this mob the higher minds are brought down to the lower level, and they are driven to consent to a course which their individual judgments condemn. The opinion of all independent men must stamp the mode of opposition adopted by the profession as not only unjust but illogical as applied to such a subject as the Doctrine of Specifics. Let us now, separating the practical expression of the profession from the arguments which they think justify this conduct, examine into the validity of the alleged objections.

In explaining what our system is we refuse to accept the appellation of homœopathists, we are medical men following where the administration of drugs is required the law of specifics, but we are bound by no dogma, and we are free to depart from that law whenever the exigencies of a disease may require it, or to adopt any better rule if the future should discover such. We employ the term homœopathic and allopathic not to persons but to things, the former to drugs whose relation is that of similarity to the disease, the latter when the relations are of any other kind. We claim as great an interest as the ordinary school in all the sciences accessory to medicine, and in those which constitute medicine. It is where the actual administration of drugs is involved that our differences commence. Whenever the art of healing is due to means other than drugs is common property; such as all that is embraced under climate, diet, regimen, exercise, rest, and surgical appliances; also such means as cod liver oil and lemon juice which we regard as dietetic agents. We also claim the employment of all anti-para-

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sitics, of emetics and purgatives, when they are administered to remove quickly some offending cause such as poisons, seybalæ, &c. We use anæsthetics in operations, and in parturition, and we may administer sedatives in exceptional cases when the spread of an incurable malignant disease, either mechanically or from disorganisation of the part, bars the action of a specific remedy. Putting aside any chemical or anti-parasitic power, all medicines must act on disease in virtue of their physiological action in the healthy body, therefore the correct knowledge of the pathogenesis of drugs must be the foundation of therapeutics.

Diseases may be treated directly and indirectly; the most direct mode of treating disease is by the employment of specifics. What is a specific? Sydenham regards specifics as the third great desideratum in medicine, and he characterises them as remedies effecting cure without causing any evacuation, and they "must directly destroy the species of the disease, and that not by introducing different qualities against a disease whose essence consists in none of them;" that is, the specific must be of a similar nature to the species of the disease. Sydenham regarded Peruvian bark in ague as the only medicine that supported his definition of specifics, and he excluded mercury in syphilis "until it could be shown by cogent process that it produced its beneficial effects without salivation." *A specific is a remedy which cures with the absorption of its whole physiological into its therapeutic action.** There are numerous instances of cure by specifics where physiological action has been very manifest, but this is not necessary to cure, and retards recovery.

The discovery of specifics met with little attention until Hahnemann, in 1791, struck with the physiological effects of full doses of bark, suggested that the relation between the physiological and the therapeutic *might* be one of similarity, and six years later he announced this as the law of specifics.

This law expressed in the formula *similia similibus curantur* is the fundamental principle of the specific school. Specific medicine has nothing in common with the name as misapplied to infallible universal remedies, on the contrary, the law of relation between the remedy and

* Dr. Drysdale, *Brit. Journ. of Hom.*

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the ailment implies that the means must be as various as are the phenomena of disease.

This possible relation of similarity between the remedy and the disease is admitted by many medical writers, but there is this difference, that Hahnemann and his school make this relation the rule for selecting the medicine, whereas the ordinary school, though they admit the fact of an occasional homœopathic relation do not act on this knowledge, but attribute such cures to some other supposed influence of the drug.

To illustrate the views of the specific school, let us choose *arsenic*, as it is a remedy whose therapeutic uses and physiological action are well known. In Dr. Headland's work, where the remedies are classed by their therapeutic results, and not their physiological action, *arsenic* is reckoned among the catalytics, on the assumption that its operation in the blood results in the destruction or counteraction of certain morbid agencies.* He observes that

"The action of catalytics which are the most potent remedies employed in disease, is involved in a greater degree of doubt and obscurity than that of any other class. How can we, for example, by any of the common terms which are made use of to designate the action of *mercury*, of *arsenic*, and of *iodine*, express the peculiarity of their influence over syphilis, lepra, and scrofula respectively. Does it not seem better to confess our ignorance, and to say that all we know for certain is that these remedies have the power of antagonising certain diseases."

But this is merely saying *arsenic* cures some skin affections—and to state that it does so by counteracting certain morbid agencies in the blood is an assumption, for there is no evidence that skin affections are due to a morbid agency working in the blood. Erasmus Wilson attributes the two affections in which *arsenic* is most useful, eczema, and alphos or lepra, to various forms of debility; and he describes *arsenic*, from its therapeutical and not its physiological aspect, as a neuro-tonic, which "seems to act specially on the peripheral and cutaneous nerves, improves their tone, and renders more active the nutrition, and restoration of the skin.† Here, then, a medicine cures

* *Action of Medicines*, 4th edit., p. 177.
† *Cutaneous Medicine*, p. 93.

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a disease, and according to the peculiar views of the writer, so is the remedy designated. Dr. Headland grants "that in some cases the action of catalytics may appear similar [to the disease] in some respects. But this," he adds, "merely shows that they move as it were in the same sphere, for else one of them could not meet the other. However the operation of a medicine may sometimes seem to resemble that of a disease, it always is in effect contrary to it. *The similarity is of a kind that does not concern us.*"

We fully grant that the curative effect is contrary to the disease, and, did time permit, it could be shown that the similia is in reality the true contraria. But we maintain that the similarity is of the utmost consequence in selecting a specific drug, for it leads us from simple empiricism to assigning a definite general law which expresses a constant relation between the many catalytics and alteratives and the diseases in which they are useful. Dr. Headland says *arsenic* "deteriorates the blood, and causes eruptive disorders." (p. 185). This action, which he considers of no value, is the true clue to the specific action of *arsenic*, and can be demonstrated to exist in all the diseases in which general experience attests the validity of *arsenic*, such as ague, convulsive diseases, cutaneous affections, rheumatism, neuralgia, &c.; and no other relation between the drug and these various affections can be pointed out. It is this relation which gives the therapeutic value of *arsenic*, in so great a variety of morbid actions, its scientific basis. It is this general law which gives the power of predicating the value of a remedy whose physiological action is known in a new disease, or one in which it has not hitherto been given. It was this which led Hahnemann to assume that *camphor*, *arsenic*, and *veratrum* would be useful in cholera, and experience has amply confirmed the truth of his forecast.

Such, then, in brief terms, is the fundamental principle of the Specific school, and we now ask how can such a principle be repugnant to common sense, and opposed to science?

Is the conception of a general therapeutic law unphilosophical? Is it contrary to science to be dissatisfied with the conjectural, and to seek after the fixed? To assert that therapeutics ceases to be founded on ever-changing hypothetical views, and now claims to take rank

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among the inductive sciences? Viewed simply as an abstract scientific statement, does it present anything unnatural, or is it in discord with what is known of physiology and pathology? To what established law is it contrary?

It may perhaps be answered, that it is not the statement of a supposed therapeutic law which excites opposition, but the absurdity of the minute doses adopted by the Specific school.

On the subject of the dose it must be admitted there have been faults on both sides. While the dose has been caricatured and often misrepresented by our opponents, it has been stated in exaggerated terms by Hahnemann, and unphilosophically received by some of his enthusiastic followers. The subject of the dose is a very difficult one, but stated simply and independently of any theory, it may in some instances appear to be *not contrary* to but *only beyond* the experience of the ordinary school.

Hahnemann for many years employed the ordinary doses of drugs which he gave as specifics in accordance with his therapeutic rule. Then, to avoid physiological action, he gradually reduced the dose. As he grew older his posological views became more hypothetical, and at last occupied in his mind a far greater prominence than the therapeutic law on which his fame must rest. Such views were far from being generally received, and discussions on this point have always occupied a large portion of homœopathic literature. There is no necessary connection between the dose and the homœopathic law, the one exists quite independently of the other. As specifics are remedies which can cure without exciting physiological action, the dose lies within the limits of that action. A dose is sufficient when it stimulates the diseased parts to healthy action without the occurrence of chemical or physiolgical action. What the dose, necessary to this end, is, can only be ascertained by experiment, as it is dependant on the particular remedy, on the susceptibility of the diseased organ, and the idiosyncracy of the patient. To understand the minute doses generally employed by the Specific school, they must not be disconnected from the circumstances in which they are given. It is an error to suppose that the amount of force required to originate a change is the same as that necessary to modify that change when already present. The direction in which a power is applied is much more significant than the power

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itself. A much smaller force is necessary to deflect than to propel a projectile. Unorganized nature is a constant commentary on the error of reducing every manifestation of force to the standard of weight and measure, and organized living structure is still more sensitive to reactions of every kind. It is a fallacy to suppose that the therapeutic value of a specific remedy increases in the ratio of its quantity. This is readily shown in the action of *mercury;* much which is given is surplus, and in reality the actual quantity of *mercury* which is useful as an alterative may be very close to the lower doses of the Specific school. Though large doses of specifics are given in ordinary practice, much of this quantity is chemically decomposed, and a greater quantity rapidly eliminated, and that often in proportion to the amount of the dose—so that the really potential dose is but a fraction of the actually-administered quantity. If carefully studied, and without prejudice, it will be found that the generally minute doses of the Specific school are not contrary to the experience of the ordinary school, but merely without the circle of their experience, because the experiment has not been tried by them. Let the experiment be made by them of gradually reducing their doses of specific medicines, and we are confident that the generally minute dose of the Specific school will not appear extravagant.

Let us again take the instance of *arsenic.* Why in ordinary practice is it so generally administered on a full stomach? Were it not better to avoid irritation by giving a smaller dose? "It must be remembered," says Erasmus Wilson, "when prescribing *arsenic* that time and small doses are more curative than larger doses continued for a short period."* His dose of arsenite of soda is from the $^1/_{20}$th to $^1/_{12}$th of a grain. Sir James Simpson gave it often in doses of the $^1/_{60}$th to the $^1/_{100}$th of a grain. Boudin, an experienced medical officer in the French army, "often obtained, by a single dose of the $^1/_{100}$th of a grain of this medicine, the radical cure of a fever contracted in Algeria or in Senegal, and which had resisted means of various kinds, including sulphate of quinine, and change of climate." †

Mr. Hunt, as long ago as 1847, in his treatise on

* *Cutaneous Medicine*, p. .
† *Traite des Fievres Intermittentes*, p. 277.

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Skin Affections recommended extremely minute doses of Fowler's solution, and the *Brit. and For. Med. Rev*, in their notice of the work, accept Mr. Hunt's experience as genuine, and remarks that each dose contained only the $^1/_{480}$th part of a grain of white arsenic. All the doses now mentioned are used by the Specific school; and if their experience leads them to give still smaller quantities, what just ground does this afford for excommunication? It is unphilosophical to make a question of dose a plea for anathema. No *à priori* reasoning can prove, on the ground that because $^1/_{20}$th of a grain of *arsenic* is sufficient, that therefore the $^1/_{16}$th, or $^1/_{100}$th, or $^1/_{480}$th of a grain must fail, and if the 400th succeed, why may not the 1000th or 10,000th? It is simply a question of experience—and the testimony in favour of the small doses has been given for half-a-century by thousands of skilled witnesses. By witnesses who give their testimony, fully recognizing the uncertainty of medical experience, and the worthlessness of masses of supposed therapeutic facts, allopathic and homœopathic, furnished by the *post hoc* being mistaken for the *propter hoc*. Their belief in minute doses did not suddenly flash on their minds from the stimulus of a credulous imagination, but on the contrary they had the sway of long-established teaching and habits to overcome, and this was only conquered by the irresistible testimony of carefully observed cases.

Some justify the excommunication of our school on the grounds of breach of medical ethics, an excuse utterly without foundation. We hold that a regular medical education is the only presumptive evidence of professional acquirements; and it is the only acknowledged right of an individual to the exercise and honours of his profession. These rights and honours he is entitled to enjoy as long as he maintains the dignity of his profession, and strives to extend the bounds of its usefulness.

Medical ethics have no other foundation than these, and we challenge those who excommunicate to show when and how we offend in these particulars. Let them take now the oldest medical quarterly journal in England, the *British Journal of Homœopathy*, which has expounded for thirty years the principles of our system. We defy the keenest of our opponents to find in those thirty volumes one statement which can sustain his argument. It is not obligatory on us to defend individual practitioners: there

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may be men practising homœopathically who may be quackish, just as there may be men practising in the ordinary way who are thorough quacks; but it would be as unjust to condemn allopathy on these grounds as it is unfair to homœopathy to charge it with the ethical faults of individual practitioners.

From time to time some men of candour and eminence in the profession have protested against the injustice of ranking homœopathy among the list of quackeries; and lately Dr. Rumsey, a physician well known for his general liberal culture, and professional experience, has shown his moral courage and his candour in his opening remarks to the British Medical Association. Dr. Rumsey says:—

"We are often tempted, perhaps unfairly, to apply the term 'quackery' to the practice of medical novelties or heresies. Now, I would suggest that the utmost freedom of judgment and action in the selection of means for the prevention or relief of sickness may be allowed by the authorities, and may be employed by members of the profession, without incurring an unmerited or degrading nickname. There are quacks as regards their conduct, who may be most orthodox as regards their theories of medicine. On the other hand, there are learned medical sectarians, to my knowledge, who, in the exercise of their calling are honourable and upright. That real quackery, which is to be found even among the ranks of the 'regulars,' and which consists in loud pretension, unwarrantable assumption, pomp of equipage, and extortion—may be checked, if not repressed, by two methods. The one is in our own power, but it requires the cordial assent of the authorities and members of the profession to some definite code of ethics. The other depends on the view taken by the Government and legislature of our relations with the state."

We believe there is as large an amount of candour among those who oppose us as there is in any profession, that virtue latent as regards Specific medicine we desire to stimulate into action by showing how different our system is from that which is pictured by professional prejudices.

The opposition as yet conducted has simply retarded, not arrested, the study of Specific medicine, and it will fail—for it is merely an attempt to overcome those who might otherwise venture to examine; and it reminds us, in its nature and its failure, of the old saying: "Out of Gallilee cometh no prophet," the aim of which was to exclude the consideration of unanswerable claims. It is

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only an honest, thorough examination which can put the profession in a position to speak authoritatively as to the failure or excellence of the system of Specific medicine.

The Chinese plan of shutting out the Barbarian will not answer. Why, by all that is reasonable, do you, our opponents, excommunicate those who examine, when according to your views such examination ought to prove the errors of our system? If you have confidence that truth may be trusted in virtue of its own power to survive any amount of investigation, then why deny to the Specific school the right of open discussion? If you will not personally investigate, why deter those who have examined from expressing their opinions in the pages of your journals, or prevent our periodicals from circulating in your libraries, or our advertisements appearing in your medical circulars?

As men sincere in convictions, matured by years of practice, we appeal to our opponents on behalf of therapeutics. We simply ask for free and open discussion. We desire nothing but a fair field and no favour.

Your journals do occasionally introduce the subject of homœopathy, but only to traduce it; you admit of no reply—you will not listen to any statements from men capable of speaking practically on the system. You attempt to keep us out, but you hedge yourselves in with intolerance.

The appeal to examine you cannot treat by the cry that life is too sacred to hazard on such a trial; such an excuse might have soothed a prejudiced mind in the days of heroic medicine, but in these days of triumphant expectancy it is valueless. If our system of treatment is, as you say, the consummation of the plan of doing nothing, aided by an admirable code of diet and regimen, then it is the distinct expression of the present practical tendency of the most enlightened medical men of your school; and this negative good ought to recommend the system for further examination, lest it contains the positive good which we maintain it possesses.

It is unnecessary to allude to the changes in heroic medicine which the results of the Specific school have brought about; but in addition to this negative good, shown in the rise of the expectant school, there are direct effects noticeable in the increasing employment of specific medicines by the ordinary school. Good results

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are also flowing from a more correct knowledge of physiology, due principally to the teachings of Fletcher, of Claude Bernard, of Brown-Sequard, of Beale, and others, views which point irresistibly to the therapeutic principles of Hahnemann, and which are leading to the gradual abandonment of the chemical and eliminative theories, which have so long retarded the progress of therapeutics.

To these physiological views we are indebted for two able converts, Drs. Reith and Dyce Brown, the former wrote three very able papers in the *Edinburgh Medical Journal*,* and was not a little startled when he was told that the conclusions he had come to were the essence of homœopathy. He could not believe it at first, but after reading some of the works of homœopathic medical authors, he was obliged to confess that the ground he was proposing to occupy was already filled, and that the field he proposed to enter had been well cultivated by Hahnemann and his followers. "They had entered it by one way, while," Dr. Reith says, "I had come from another—that was the whole difference. I thus occupied the singular position of being brought face to face with homœopathy simply by following out the teaching of some of our most eminent writers, and not by studying the system itself."

But there are adoptions of specific medicines where owing to the traditional dislike or ignorance of homœopathy no acknowledgement is made of the source from which they were chosen, or it is alluded to in contemptuous terms, and the evil of such adoptions is that the remedies are administered empirically, and not in agreement with any general law; *e.g.*, the curative effects of *mercury* have been for ages assumed to depend upon its supposed action of increasing the flow of bile. A committee was lately appointed to examine and report on the point, and their conclusion is, "we do not deny the possibility of *mercury* being useful in some diseases of the liver. We simply say that the notion of its doing good by increasing the biliary secretions is untenable." Had the committee consulted the works of the Specific School, and not obstinately excluded them from their investigations, they would have found that fifty years ago Hahnemann made the same announcement.

Had Dr. Harley been acquainted with the same writings

* Feb., Ap., Sept., 1867.

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he might have enriched his very interesting and valuable examination of the old vegetable narcotics; he must then have seen that he has thrown no new therapeutic lights on the uses of those remedies, and he might have been spared making what he considers an original announcement, "the action of *belladonna* in febrile diseases is frequently attended with results which are not only unexpected, but exactly the opposite of what is observed in health." Why this is the A B C of homœopathy, of which we conclude he knows nothing. The curious part is that the illustration of *the mutual extinction of waves of similar periods* used by Dr. Harley has long been familiar in homœopathic works, and was first used by Dr. Drysdale in 1846, and in remarkably similar words to those given by Dr. Harley.*

Mr. E. Heath recommends as a novelty small and repeated doses of *belladonna* in erysipelas, and he thinks it might be usefully had recourse to in cases of congestion of the brain from any cause.† He is ignorant that for more than half a century the Specific school have given it in congestion of the brain, and he forgets that many years ago Mr. Liston the eminent surgeon testified to the good he had seen this medicine produce in erysipelas when given in minute doses by homœopathic practitioners.

But it is needless to enumerate the rediscoveries which are now being made of specific medicines long known and used by the homœopathic school. So numerous are these rediscoveries now becoming that for the credit of ordinary medicine, and for the saving of much time and disappointing of would-be original observers, it were prudent to remove the works of the Specific school from the Index Expurgatorius in which the profession has long included them.

If these supposed discoveries go on in their present ratio, the time is not far distant when it may fare with Hahnemann's discovery much as it did with Columbus' egg. The doctors of Salamanca could make the egg stand after Columbus had knocked the end in, and so it is not unlikely that in time Hahnemann's discovery will turn out no discovery at all, and the present hue and cry will change into the assertion—"Oh, we knew all this before."

* *Introduction to the Study of Homœopathy*, 1845, p. 180.
† *Practitioner*, Dec., 1868.

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One quotation must be given as illustrating the lack of generosity and the absence of moral courage which can be manifested by men of science and high professional position, when they succumb to the incubus which the prejudice of the profession has created.

Dr. Wilks, in a short article on *aconite* and its uses in inflammatory fever, makes this remark:—

"As regards *aconite*, I am acquainted with two medical men who in the course of a long practice have been in the habit of daily using it, but have not cared to speak of it too openly for fear of having their names associated with members of an eminently quack system, and it may be mentioned that the late Mr. Liston brought no little odium upon himself on account of his advocacy of the use of this drug in erysipelas" (p. 33, 34).

As an example to Dr. Wilks who panders to vulgar prejudice by insulting a class of men who claim a culture and an experience equal to his, and to the two friends, let them read the testimony given not by a homœopathic practitioner, but by a professor of Materia Medica in the University of Gratz. "It was Hahnemann who first recommended the use of *aconite* in pure inflammatory fevers, with or without eruption, as well as in inflammatory diseases generally, in obedience to his principle, *similia similibus*, by which the effusion of blood, except in certain exceptional cases, is wholly obviated. Even were we under no other obligation to Hahnemann, by this simple discovery he would, like Jenner, deserve to be ranked among the greatest benefactors of suffering humanity."

Alas, that there should be so many who, like Dr. Wilks, and his timid friends, shrink from the avowal of truth because the way looks thorny and likely to be troublesome. Alas, that members of a liberal profession do not feel that such direlection of duty inflicts a serious injury on their own art, the more so when not satisfied with filching the discoveries of Hahnemann, and arraying themselves in his hard earned honours, they attempt to cover their plagiarisms by abusing the discoverer and his school.

How different was the manly conduct of one whose late death we have deeply to mourn. One who felt that truth can only be gained by magnanimity and self-denial. How nobly Professor Henderson battled for the truth. With a masterly knowledge of his profession, he came to

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the examination of the question suggested to him by seeing the good effects of homœopathic treatment in some of his former patients. He felt that it was a crime not to examine fully, and when convinced of the utility of the homœopathic law he walked on,

"Attended
By a strong-siding champion, Conscience,"

resisting all entreaties to pause, he sacrificed position, friends, and peace, in the determination to witness boldly to what he believed to be a great truth. Associated with him in his early practice of homœopathy, I know how sanguine he was that the profession would ere long seriously examine, and follow his example. In this hope he was to be disappointed; his generous call to examine was met by the most rancorous attacks which worked their end, not in refuting but in preventing the profession examining into homœopathy. Attacks so scandalous that one of the principal authors on his deathbed sent asking for Henderson's forgiveness.

Let us ever encourage the hope, strongly held by Henderson, that when the candid and intelligent of both sides come to know one another, and to understand each other's views, they will discover that they are not so far apart as they at present suppose. Ajax in his fight prayed for light, let our unceasing earnest prayer be too for light, to dispel the clouds and dust of controversy, so that the combatants on both sides shall more correctly recognise each other, and honour one another's sincere thought. This gained, the result will be that the judgment and conscience of each man shall without censure be his therapeutic guides.

Let us now consider the good which Specific medicine may gain from this constant opposition. Were this opposition the result of calm investigation and of a practical trial of the system, then what is true would be elicited, and what is error exposed. Unfortunately the opposition has no such character, and therefore fails to eliminate either the good or the bad. It must therefore be considered simply in its effects as forcing us to diligently cultivate the points which are characteristic of our system.

In the gradual change taking place in ordinary medicine shewn in the increasing employment of specifics, it is of great consequence that a school exists which puts pro-

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minently forward as its fundamental belief a general law which rescues these specifics from simple empiricism, and gives them a scientific basis. The tendency to routine is the fault of practical medicine, it engenders the evil of pressing species into genera, of treating nosologically instead of considering the special pathological conditions presented in the individual patient. How often two cases of the same disease which agree in a few promient features differ materially in their special character. These differences are so great and so numerous, as to make Dr. Abercrombie remark that "those who have had the most extensive opportunities of observation, will be the first to acknowledge that medical experience must in general sink into analogy, and even analogy too often into conjecture."

Hahnemann has very clearly pointed out that "with the exception of a few peculiar diseases, the innumerable remainder are so different that each of these is seldom observed more than once; and each case which we meet with ought to be considered and treated as an individual disease which has never yet appeared previously as we see it in the present case, under like circumstances, and is never perhaps to reappear exactly in the same form."

We cannot too strongly insist on the value of individualising. Due importance ought to be placed on the subjective symptoms both as occurring in the patient and in the pathogenesis of the drug. This class of symptoms is not unfrequently the clue to specific differences, and often assigns the true pathological value to the objective phenomena.

As we believe a knowledge of the physiological action of drugs is the true basis of therapeutics, we have a duty to perform in advancing this knowledge, and in keeping it steadily before the profession. There is no better field than the careful proving of drugs for the cultivation of that exact and minute detail which is the true source of success in specific medicine. Feeling what results have flowed from our as yet imperfect knowledge of the action of drugs, we must seek for numerous medical converts in order that this rich mine be diligently and successfully worked.

It is fortunate that the important subject of the action of drugs is to be discussed to-day by so able and experienced an observer as Dr. Sharp. The opposition assails

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us on the question of the dose, and the attacks will last until the profession recognise with us that doses short of producing physiological action are the safest, and the best in specific treatment.

But while the charm of old habits drives our opponents to view this question with prejudice, let us take care that the charm of novelty, an equally seductive mistress, does not tempt us to extravagance or narrowness in considering the dose. It is this question more than any other which may lead some of our enthusiastic colleagues to drift into Sectarianism, a position we must never be driven into; we must promulgate our belief not as sectaries but as catholic members of the universal school of scientific investigation. We must rise superior to every suspicion of being a sect, by cultivating a true eclectic spirit, by readily appreciating good wherever found.

Our belief forms but a portion, though an important part of the Healing Art, and our sanguine hopes are not to revolutionise medicine, but to leaven it with the law of specifics. That law once generally admitted, the question of dose will be all the sooner determined. The more general the belief becomes in the therapeutic law, so in proportion will exclusiveness as to dose diminish.

We disclaim being healing monopolists; there are numbers of possible lines between two points, although the shortest be but one. We think we follow the shortest, but let it be proved that there is a still shorter and better, and we must be ready to adopt it. Beliefs proved contrary to fact must be at once and heartily abandoned.

We disclaim being infallible curers; where cure is possible we think our system gives us important facilities, but a right study of it also teaches how short we must often come of our best aspirations when we remember the difficulties which arise in drawing a true pathological simile.

In defending our cause we must only turn aggressors when self defence renders it necessary, and our argument must ever be conducted with courtesy, and without appealing to the prejudices or passions of the ignorant.

It is our duty boldly and perseveringly to claim the liberty of free opinion and the right of choice, which are the heritage of all members of a scientific profession. We claim admission to all the rights and honours of our pro-

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fession, and as the condition of such rights we invite the strictest scrutiny, we demand a fair field and no favour.

We are perfectly content to let our system stand or fall by the test of experience; we have that confidence in its value, that tried by such a standard we know "it must soon be utter night with a lie, or glorious sunshine with a truth."

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IN WHAT WAY IS THE ACTION OF DRUGS TO BE DISCOVERED?*

By William Sharp, M.D., F.R.S.

"The qualities of bodies are known to us only by experiments."
Sir Isaac Newton.

Nothing more clearly betrays the extent of the weakness and infirmity into which the human mind has fallen than the perverted ingenuity it has shown, and the mistaken ways it has followed, in respect to the ends at which it has aimed.

On most, if not on all subjects which man's mind has attempted to investigate, many erroneous methods, probably all possible ones, have been pursued, before the true method has been found.

We shall see an eminent illustration of this fact if we now consider the action of drugs, and take up the first question proposed, namely, in what way is the action of drugs to be discovered?

If we look back through the avenue of five-and-twenty centuries, we shall find that the knowledge of the action of drugs has been sought for in very various ways. We shall see that some of these ways are manifestly wrong, and that the rest are more or less imperfect. The orthodox school of medicine acknowledges that all have faile

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These various ways are as follows:—

1. Drugs have been regarded superstitiously.
2. They have been viewed astrologically.

* This is one part only of the three into which the Essay on the Action of Drugs has been divided. Part II., "What is the Action of Drugs?" and Part III., "How is the Action of each Drug to be distinguished from that of all others?" have already been published. Of the twelve chapters of which this Part consists, time would permit the reading at the Congress of the last chapter only, which is given here. The complete Paper will be published in the *Monthly Homœopathic Review* in a few months.

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3. They have been studied with reference to their sensible properties—such as form and colour, taste and odour.
4. Drugs have been studied chemically.
5. They have been studied mechanically.
6. They have been studied botanically.
7. They have been studied pathologically.
8 They have been studied empirically.
9. They have been studied mainly for their indirect action.
10. Experiments on animals.
11. Experiments on the sick.

The time allotted for the reading of Papers at this Congress will not allow me to ask your attention to more than one chapter; I therefore remark that the conclusion arrived at, in the examination of each of these ways, is that it has failed; and proceed to read of another way, which has led to much more success in the treatment of the sick.

12. *Experiments on the healthy.*

The truest vindication of these experiments, and therefore the most becoming introduction to the subject, is contained in the first sentence of "The Great Instauration" of Lord Bacon:—

"Francis of Verulam thought thus—

"Of the state of learning—That it is neither prosperous nor greatly advanced, and that *an entirely different way* from any known to our predecessors must be opened to the human understanding, and *different helps* be obtained, in order that the mind may exercise its jurisdiction over the nature of things."

If we substitute the word "medicine" for "learning," the sentence will still be true of this branch of knowledge. It is the unanimous confession of those best acquainted with medicine, that, as a science, it is "neither prosperous nor greatly advanced," though it has not hitherto been their conviction that "an entirely different way" must be opened, if it is to make any great advance. Without experience to the contrary, it would have seemed natural that such a conviction would necessarily follow the confession, but this has not happened.

It is the truth of this sentence of Lord Bacon which

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justified Hahnemann, and which justifies us, in his, and in our endeavours after the discovery of an entirely different way from any known to our predecessors.

And the way proposed and entered upon by Hahnemann, and pursued by ourselves, is the way of learning the properties of medicines by experimenting with them, not only on the sick, but also on the healthy.

As two Essays (on the "Proving of Drugs," and on the "Physiological Action of Medicines,") have already been occupied with this subject, I will content myself, on this occasion, with offering only a few remarks.

1. Let us not aim at impossibilities. In undertaking these experiments on the healthy, it is of great importance that we limit our endeavours within the bounds of what is possible to us. If we neglect this precaution much time and effort will be thrown away, and we shall reap disappointment. For example, if we propose to ourselves to find out the *manner in which drugs act*, we shall soon get out of our depth, and our labour will be lost. I know that some of you do not agree with me in this persuasion. The discussion of it cannot be entered upon now, but I commend to you the following sentence of Sydenham:—

"However much, by seriously inclining our minds, we may discover what nature does, and by what organs she does it, the way in which she does it, will always be unknown to man."*

2. Let us not stop short of what is possible to us. It is not surprising to find that the first efforts to make progress in a new path are defective. And many of you are now disposed to agree with me that the limitation of these experiments on health by Hahnemann and his followers to the enumeration of symptoms is not doing all that it is needful to do. For, if on the one hand it is a mistake to attempt what is beyond our power, it is, on the other hand, also a mistake to neglect to aim at doing all that is within our power. It has often been contended in these Essays, that when experiments with drugs in health are undertaken, besides the phenomena or appearances produced, called symptoms, being noted, the seat of these symptoms, or *the organs to which they belong*, should be

* *Sydenham's Works*, by Sydenham Society. Vol. II., p. 84.

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noted also. It is freely granted that the connection between a symptom and its origin may sometimes be very difficult to trace, but you will not argue that it is impossible to trace it—that it is beyond our natural powers; and if it be granted that it is within our power, it must immediately be granted that it forms part of our duty.

3. We are also pursuing "an entirely different way from any known to our predecessors," in another sense. It is earnestly contended that, with very few exceptions, the only action of drugs which is required of them, as remedies in disease, is that which they perform "silently and peacefully." This is the action which was formerly called "alterative," and now "specific." These terms imply two things:—that it is unaccompanied by signs perceptible by our senses; and that we know nothing of the manner in which it is performed.

The general adoption of this method of prescribing drugs would bring about a greater revolution in the practice of physic than has ever yet been dreamt of. And yet it is obviously in the right direction. "Nature is pleased with simplicity," said Sir Isaac Newton, and "more is in vain where less will serve." All real improvements in Art are in the direction of greater simplicity.

When it is considered that this method leads to the ignoring of all established indications; to the renouncing of all former intentions; to the laying aside all that is usually called "active" treatment—it is not surprising that it is very repugnant to the majority of the profession, or that from them it meets with very determined opposition.

To see in our future works on Materia Medica no more catalogues of drugs arranged under the heads of "emetics" and "purgatives," "diaphoretics" and "diuretics," "sialagogues" and "deobstruants," is an anticipation too astounding to be contemplated with equanimity—too impossible to be realised. But this is the future which we humbly hope is before the profession.

And when the prejudices of education and the power of present habits have been overcome by the force of truth and the evidence of facts, what a beneficial change will have been brought about! Medicines, instead of being nauseous draughts, will have become pleasant charms;

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and physicians, instead of being shunned and dreaded, especially by children, will be welcomed and loved.

This silent and peaceable action of medicines, secret and hidden from our knowledge as to its manner, but very visible in its beneficial effects, is in the second sense, *as a principle*, the "entirely different way from any known to our predecessors," which it is our happy privilege to advocate and defend. Instead of all the perturbative methods of the past, this is the curative method of the future.

4. The experiments with drugs in health are the "different helps," which Lord Bacon says must be obtained, in order that this entirely different way may be opened.

We want to learn of a drug, not whether it is a purgative or a diuretic, but what is its specific action—that action which is a disturbing action in health, and a silent and peaceful action as a remedy in disease. Experiments in health are *helps* in the acquirement of this knowledge. The specific action of a very few drugs has been discovered accidentally. The experiments in health which have already been instituted have helped to increase this number greatly; a continuance of them may reasonably be expected to add many more to the list, and to make our knowledge of them much more perfect.

5. The help which Hahnemann got from these experiments was obtained by observing the similarity of the *symptoms* produced by them to those of diseases; and he prescribed according to this similarity.

The help which I am now seeking to obtain is by observing the *seat* of the action of drugs, and its identity with that of diseases; so that a drug may be prescribed which has its action where the disease is principally situated.

There cannot be a question that this is more definite knowledge than that obtained by the mere observation of symptoms. And if more definite, then in the same proportion must it be more valuable.

6. When this identification of the seat of the action of the causes of disease, with the seat of the action of each drug, has to some extent been accomplished, a further

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help may be sought from experiments in health, by the discovery of the similarity and contrariety in the *kinds* of action of diseases and drugs.

After observing the symptoms, or signs of an action; and after discovering its seat, or the organs in which it is taking place, we may try to learn what sort of action it is. This is a further step in advance, and consequently the difficulties will increase.

In the study of nature our first task is the observation of facts. This belongs to our bodily senses, and the value of the performance of the task is determined by its accuracy. The difficulties which beset the duty, are the imperfections of our senses, and our hastiness in using them. Our next task is the interpretation of these facts, the learning their true meaning. This belongs to the mind. The difficulties which hinder its being rightly performed, are the weakness of our mental faculties, and the strong tendency which exists in us to invent hypotheses, that is, to guess at interpretations, rather than wait till the mind can observe them.

When, therefore, we endeavour to discover the kind of action which a disease or a drug is producing in an organ, we cannot exercise too much caution. The observations must be wary and prolonged to learn the facts; and it is absolutely necessary to confine ourselves to the use of words which simply express the facts observed, and which do not suggest any hypothetical explanation. For instance, if we see vessels become smaller or larger than they were before, instead of calling this a stimulant, an astringent, or a relaxant effect, let us be content to say of the vessels, as we say of the pupil, that they are contracted or dilated. The same caution is, if possible, still more imperatively required, when the mind attempts to interpret the meaning of the facts.

7. It may be noted in this place, that the fact which lies at the foundation of the difference between the disturbing action in health, and the peaceful action in disease, is the difference in the dose. Organs in a state of health are, generally, not disturbed by a dose which is found by experience to be sufficient to act upon them curatively in disease.

Different doses of the same drug sometimes act upon different organs of the body in health; but doses, smaller

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than those given in health, can always be found, which act upon the corresponding organs in disease.

The subject of doses is a wide and interesting field for dispassionate and very careful enquiry and observation; but it is one of extreme difficulty and obscurity. The motto for it Lord Bacon has given us in these words:—

"In all our investigations of nature we must observe what quantity or dose of the body is requisite for a given effect; and must guard ourselves from estimating it at too much or too little."

8. Let us also note, once more, the direct object and the use of these experiments in health. They are these:—

First. To learn the action of each drug by itself; unmixed with other drugs.

Second. To learn the action of drugs uncomplicated with the symptoms of disease.

Third. To discover the specific action of each drug; that is, to learn the organs upon which it acts, and the kind of action.

Fourth. To apply these discoveries to the treatment of disease; experience having taught us that the same organs which are disturbed in health by certain doses, are silently and peacefully cured in disease, by certain smaller doses.

9. Let us observe the indirect uses of these experiments. One branch of knowledge can generally throw some light upon another, and this indirect use is not wanting in experiments upon the healthy. If we take the drugs which are known by experience to have a useful specific action in a particular disease, and make comparative experiments with them in health, we are sure to gain some information as to the seat or nature of the disease in question.

I may suggest, in illustration of the last remark, that if we were to take up gout, and examine the drugs given for it as remedies, it is obvious that, at starting, we must lay aside the remedies given on the evacuating plan; we must also lay aside such disputes as whether cold purgatives, like Epsom salts, or warm ones, like jalap, are to be employed; or, whether alkalies or acids are to be preferred; and, for the present at least, we must lay aside the tentative

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experiments of the chemists, such as the benzoic acid of Mr. Ure, the alkaline lithia of Dr. Garrod, and the phosphate of ammonia suggested by Liebig. Only the specifics remain; these may be taken, and a comparative examination be made, in order to discover what there is in common, in their action in health. The reflex benefit would be a better knowledge of the nature of gout.

The drugs are such as *bryonia*, *colchicum*, *rhus*, and *cinchona*: *pulsatilla*, *nux vomica*, *rhubarb*, and *sulphur*. Let me propose as a subject of study, the question—How far do these drugs agree in their symptoms, and in the seat and nature of their specific action?

In the same manner the principal known remedies for other ailments may be studied, *e.g. ipecacuanha*, *sambucus*, and *arsenic*, for asthma.

The curative action of even a single remedy in a disease, the pathology of which is obscure, may throw considerable light upon that obscure pathology, simply in consequence of the experiments made with the remedy in health. To my mind *chamomilla* has done this for some cases of diabetes.

10. Another indirect use of experiments with drugs upon the healthy. It awakens attention to the possible difference of cases whose symptoms are similar. It is a fact, that cases of disease present themselves, the symptoms of which are so similar that they may readily be considered cases of the same disease; but the causes of which are so different that, to confound them in this way, would be to make a serious mistake. For example: *belladonna* may be so taken in health as to produce all the symptoms of scarlet fever. This has often happened. But this similar of scarlet fever has never been communicated to others in the manner that real scarlet fever is. It is wanting, therefore, in the infectious or contagious element, which is the true cause of genuine scarlet fever.

11. It may be worth while to repeat that the information sought from these experiments is derived mainly from two sources. The more severe effects of drugs are learned from cases of poisoning, in whatever way brought about; the less severe from voluntary provings. On this account Christison on poisons, and other similar publications, are very valuable, in a direction not contemplated by those writers on 'Legal Medicine.'

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12. Some of the effects of drugs can be obtained by the topical application of them; and these become striking proofs of local action. For example:—the pupil of one eye may be dilated to the uttermost by *belladonna;* and at the same time, in the same person, the pupil of the other eye may be contracted to the size of a small pin's head, by the *Calabar bean.* But generally the experiments are made by the drug being taken internally. In many instances the effects are the same whether the drug is swallowed, or introduced through a wound in any part of the body. On the other hand, of animal poisons, it is remarkable that even those which are most deadly when inserted by a wound, may commonly be swallowed without any injurious effect.

13. Many drugs act powerfully upon more organs than one; and the more there are of points of contact between the drug and the disease, the greater is the confidence with which it may be prescribed. For example:—for a feverish headache *belladonna* may be given as a remedy. If, in addition to the headache, there is an inflamed conjunctiva, the *belladonna* may be given with increased expectation of good. If further, the throat is inflamed, the probability of success is still greater. If, to all these symptoms there is added a bright scarlet rash, the probability becomes almost a certainty; provided that the inflammatory condition of these several parts is not complicated with grave symptoms of some other kind, which may, in fact, be such as to change essentially the character of the case.

In like manner, a patient suffering from colic, or spasmodic pain in the bowels, may be relieved by *nux vomica.* If there are also cramps, or twitchings in the extremities, it will almost certainly succeed. So, in cases pointing to *ipecacuanha,* if, in addition to the other symptoms, there is nausea or sickness, it may be given with great confidence. Again, some affections of the heart are cured by *spigelia;* the cure will be more likely to be effected, if there is also in the case neuralgic pain about the eyes.

14. These remarks cannot be concluded without the expression of regret that many provings of drugs have been published, in which a great want of seriousness and distinctness of object is apparent. Others are disfigured

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by a multitude of insignificant sensations and observations calculated rather to bring disgrace upon the physician, than benefit to the patient. The undertaking is one which should be promoted by every member of our body, but it should be warily done, with a clear purpose in the mind, with thoughtful gravity, with active suspicion of error, and with freedom from bias towards any foregone conclusion.

We are now able to answer the question with which this Essay commenced. How is the action of drugs to be discovered? Not in any of the ways hitherto commonly pursued: but in a new way. First, by experiments made upon ourselves and our friends while in a state of health. By these experiments we learn the power which drugs possess to *disturb* the health of the different organs of the body; and also how each drug may be characterised, and distinguished from all the rest. And secondly, by giving them to the sick under the guidance of the results thus obtained in health. In this way we learn their *healing* powers.

The only difference, in respect to the drug, between the experiments in health, and the prescriptions in sickness, is the quantity or dose to be taken. In the experiments the dose must be large enough to produce symptoms of disturbance in some organs of the body. In the prescriptions the dose must be small enough to avoid causing such symptoms, but large enough to act curatively, though peacefully, upon the diseased parts.

Gentlemen,—Our medical authorities, and the great bulk of our colleagues, headed by the late Sir Benjamin Brodie, are not ashamed to speak of us, and to treat us, as impostors or fools. A few, who have tardily ceased to do this, have not yet moral courage to own us. It is a great and unjust indignity; and it will one day recoil with heavy severity upon themselves. Let us not desire the evil day. Let us rest assured that it is sufficient for ourselves to know that the self-denial, discomfort, and pain undergone, and the amount of time and thought expended on such experiments in health, as were begun by Hahnemann, and have been carried on since by many others, are a sufficient testimony of our honesty; and that the successful results with which these labours have been

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crowned, are a good proof of at least an average amount of intelligence and sense.

While they thus speak disparagingly of the labourers, they are not afraid to appropriate the labours. But the attempts which have been made, during the last few years, to introduce homœopathic remedies as new discoveries, or accidental observations, are surprisingly puerile. The authors must have forgotten the declaration which we have on the highest authority, "There is nothing hidden which shall not be known."

Horton House, Rugby,
August 31st, 1872.

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ON THE PLACE AND VALUE OF BAPTISIA IN TYPHOID FEVER.

By Dr. Richard Hughes.

I am to speak to you this afternoon upon the place and value of *baptisia* in the treatment of typhoid fever. Its place—that is, its relation to the stages of the disease and the sphere of other medicines: its value—that is, its efficacy, positive and comparative, in the place it is found to occupy.

The facts which have made an enquiry into these points desirable are, briefly, as follows.

In the *North American Journal of Homœopathy* for November, 1857, appeared some cases of the low fever of that country successfully treated with the *baptisia tinctoria* by a Dr. Hoyt, with provings of the drug by the same gentleman, assisted by Drs. Douglas, Beckwith, Rowley, Sapp, and S. A. Smith.

In the winter of 1861, Dr. Madden and myself, being engaged in studying the Materia Medica together, came upon *baptisia*. The evidence in its favour from both the homœopathic and the (so-called) eclectic practitioners in America seemed to us so accordant with its pathogenesis, that we determined to test it in substitution for the *bryonia* or *rhus* with which we had hitherto encountered at the outset the continued fever met with in Brighton.

Early in 1862 we had an opportunity of trying it in a well marked case, and he the veteran not less than I the novice was impressed with the power it displayed. Unlike the remedies previously named, it seemed not to control or mitigate only, but actually to break up the disease. All our subsequent experience was in the same direction; and at a meeting of the British Homœopathic Society held in March, 1863, and again in the *British Journal of Homœopathy* for July in the same year, I communicated the results we had obtained, and recorded our conviction of the abortive power of the drug over the disease.

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In 1864 was published the first edition of Dr. E. M. Hale's *New Remedies in Homœopathic Practice.* The article on *baptisia* contained the American experiences already mentioned, with some additional testimonies of the same tenor, and adduced evidence of the general anti-putrescent action of the remedy.

In 1865 the *British Journal of Homœopathy* contained two fully reported cases of typhoid treated with the drug by Mr. Harmar Smith, the results being very satisfactory; and also a farther and more detailed statement by myself of its place and efficacy.

In 1866 Dr. Madden sent to the same journal his favourable experience with *baptisia* in the gastric fever of Melbourne; and Mr. Freeman published in the *Monthly Homœopathic Review* the good results he had obtained from it in an epidemic of typhoid occurring in Kendal.

In 1867 Dr. Bayes narrated in the latter periodical a case of enteric fever in which, even at an advanced stage, *baptisia* was of striking efficacy. The same physician has recently, in a monograph on the subject which is doubtless in all your hands, summed up and discussed much of the experience above referred to, and concludes with a strong and hopeful recommendation to further use of the drug.

Lastly, Dr. E. M. Hale, in the second edition of his *New Remedies,* confirms and adds to his previous collection of testimony in favour of this remedy, giving also a new proving by Dr. Burt; and in an article in the *American Homœopathic Observer* for August, 1866,* contributes his own favourable observation of its action in an epidemic of what he calls a "bilious remittent fever, with tendency to typhoid."

Against these numerous positive testimonies, the only contrary evidence we have on record is a statement by Dr. Yeldham, that he administered *baptisia* in one case of typhoid in the London hospital without any check being observed to the progress of the disease; and a paper read by Dr. Edward Blake at the last Annual Meeting of the British Homœopathic Society,† narrating two cases of the disease treated throughout with this medicine, in which he thinks its remedial powers at any rate "not proven."

* Extracted in the *Brit. Journ. of Hom.* for Oct. 1866.

† Now published in the *Brit. Journ. of Hom.* for Oct., 1872.

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I think, then, that there are sufficient grounds for farther enquiry. Typhoid fever is a disease of such frequent occurrence and such ghastly mortality: we have so recently escaped, as by a hair's-breadth, the loss from it of the Heir-Apparent of this Empire: it has, even when not fatal, so lengthened a process, so tedious a convalescence, such frequent *sequelæ* of even direr import than its own,—that any professing addition to our power of controlling it imperatively demands examination. Especially is it so when the promise held out is of more than mitigation of severity only, more than sustainment of the patient,—when it is of actual abortion and breaking up of the disease. If *baptisia* prove to be the aconite of this fever, we shall without controversy have gained a priceless remedy.

That it is so, I have repeatedly testified to be my own experience and conviction. But my present object is not to bring forward my own views on the subject, but, by laying before you the facts and arguments which bear upon it, to elicit yours. The verdict of this Congress, so far as it is prepared to give one, ought to go a long way towards determining the point at issue.

I proceed, therefore, to estimate the bearing and weight of the evidence I have referred to. But, ere this can be done, a very important question arises for settlement. The fever in which *baptisia* has been reputed is described in the records under several names—"typhus," "typhoid," "continued," "gastric," "bilious." Is it nevertheless in all the one essential fever we know as "typhoid" or "enteric"? or do some of the cases come within other categories, altering (so far as we are concerned) the point to be settled?

The four distinct forms of idiopathic fever which the labours of Stewart, of Jenner, and of our own Henderson have defined are so universally recognized that I need not dwell upon them. The only one which concerns us here is the so-called "febricula." This is described as a primary fever—not catarrhal or sympathetic—pretty closely resembling in symptoms the onset of the other forms, but differenced in this, that it rapidly reaches its maximum, and as rapidly subsides, within, at the most, five days. It has no local complication or specific eruption. Now it is quite possible that some of the *sporadic* cases where the disease has broken up under *baptisia* have been instances

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of this disorder, and four out of the fifty-three cases I have collated are perhaps invalidated on this account. But no such explanation is admissible as regards the remainder of the single cases in which it displayed curative powers, as all these were of more prolonged duration. While in the epidemics reported by Drs. Madden and Hale, it is noted that under allopathic treatment the fever lasted two, three, or four weeks, and in two cases under other homœopathic remedies (*aconite* and *bryonia*), for twelve days. That under *baptisia* it terminated not later than the fifth day is therefore no proof that it was febricula.

But is there yet another form of continued fever, resembling typhoid rather than febricula in its duration and progress, yet specifically distinct? and is it this disease in which the good work of *baptisia* has been done? This is a more difficult question. Jenner, Watson, and Trousseau seem to answer it in the negative—they think that all the varieties of fever described by the old nosologists, gastric, bilious, mucous, nervous, putrid, and so forth, fall under one or other of the four types now recognized. But the new nomenclature of the College of Physicians gives us a "common continued fever," as distinct from either typhus, typhoid, relapsing fever, or febricula, and Dr. Aitken cites several testimonies, direct or indirect, in favour of the existence of such a species. The opinion of several of our own school who have expressed themselves on the subject is of the same tenor. Thus—our lamented Russell, in his *Clinical Lectures*, writes: "I mean by 'gastric fever' a non-infectious, continued fever, which has no regular course; in which there is no eruption, and which is not attended with diarrhœa or intestinal affection." I must observe upon this, however, that typhoid is rarely obviously infectious; that its course varies considerably within certain limits; and that its eruption (according to Trousseau), is often wanting—Chomel, in seventy cases, missing it in sixteen, and several epidemics in Touraine being entirely without it. As regards diarrhœa, the same authority notes that it is sometimes (and especially in the mild form of typhoid he calls *mucous*) superseded by obstinate constipation throughout the illness. This leaves us only the absence of intestinal affection whereby to distinguish "gastric" fever from typhoid. But how is such absence to be ascertained? The symptoms during life are too indecisive to prove a negative, and a *post mortem* examination is all but out of the question.

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It seems to me that the *onus probandi* yet lies with the maintainers of a "common continued fever" as distinct from typhoid. My own observations and enquiries dispose me to think that, excluding febricula, there is but one non-epidemic species of low fever, and that is the enteric. The typhus and relapsing forms are seen only in large towns, or under peculiar circumstances. Typhoid is constant everywhere in more or less degree, having every now and then from local causes a special outbreak. It is the *endemic* fever of all countries, having many names and many varieties of manifestation, but characterized everywhere by the specific process it sets up in the peyerian and solitary glands of the intestines.

I pass now to the details of the evidence before us. I refrain from citing any general testimonies to the value of the remedy—many as there are which coincide with my own—and confine myself to actual records of cases.

I. The second edition of Dr. E. M. Hale's *New Remedies* contains seven cases of "fever" treated by *baptisia*, of which the following is a summary.

1. Called "typhus." No description given. Patient had been ill thirty-one days, and was "so reduced that her life was despaired of," when *baptisia* was given. Repeated doses caused determination of blood to the surface, and profuse sweating, after which improvement set in and continued to recovery, without further febrile symptoms. It is noted that, "immediately upon the administration of the remedy, she became quiet and fell asleep: while she had been restless and delirious for three weeks previous."

2 and 3. Called "continued fever." No symptoms given or time mentioned. But after *aconite* and *rhus* had been prescribed without much effect, *baptisia* was administered as in Case 1, and with the same beneficial results.

4. Called "typhus." Again no history, save that "a fierce delirium" was present, which rapidly gave way when, after the usual homœopathic remedies had been given for several days, and the prognosis had become unfavourable, *baptisia* was administered.

5. Called "typhoid." Commenced in a mild form, but on the 5th day exhausting diarrhœa set in. On the 6th the symptoms were—"delirious stupor, face dark red, with a besotted expression, eyes injected, diarrhœa, tongue coated brown, and dry, especially in the centre, very

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offensive breath, sordes on the teeth, fœtor of the urine and stools." *Baptisia* was now given every hour, and in twenty-four hours there was a return of consciousness, the tongue was moist, and all the symptoms improved. From this time the patient went on well, and made a good recovery.

6. Another case of "typhoid" with nearly the same symptoms. They were all relieved in twenty-four hours by *baptisia*. On changing to phosphoric acid the bad symptoms returned, but were dissipated by the *baptisia* again.

7. "Typhoid fever." On a day not specified, the reporter found "excessive drowsiness, pulse 120 and thready, lips parched and cracked, pasty tongue heavily furred, great thirst, could not give a direct answer to a simple question without his mind wandering, or falling into a deep sleep in the middle of a sentence, great delirium at night, could scarcely be kept in bed, low muttering." After less than twenty-four hours of *baptisia* the patient "broke out into a gentle perspiration, the pulse came down, and he fell asleep." In the morning he was found quite bright and rational. His subsequent history is not related.

II. We have next Mr. Harmar Smith's two cases.

8. The first began with synochal symptoms, for which *aconite* and *belladonna* were given. But on or about the 5th day of the fever there set in delirium and involuntary diarrhœa, and the tongue was baked down the centre. Rose coloured spots were noted on the 9th day, and abdominal tenderness on the 10th. *Baptisia* was commenced on the 6th day, with occasional doses of *arsenicum* for the diarrhœa. The symptoms were ameliorated from the first; on the 12th day the defervescence was complete; and on the 13th the enteric symptoms had disappeared.

9. In the second case the symptoms came on insidiously, so that it is impossible to specify the precise "days" of the fever, but from the 9th to the 11th day of the illness there was involuntary diarrhœa, delirium and incoherent talking, restlessness and sleeplessness. The pulse was 100 and feeble—the tongue covered with a thick brown crust. Retching and vomiting had been troublesome symptoms from the first. *Arsenicum* had been given during these days, and with some benefit, but a much more rapid improvement ensued on the substitution of *baptisia*. "There

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had been," writes Mr. H. Smith, "a succession of restless nights, which had reached their climax on that previous to the day on which the *baptisia* was commenced; but it appeared at once to set aside that morbid irritability of the nervous system which prevented sleep—thus acting like a narcotic without its unpleasant concomitants. There was no renewal of the retching and vomiting, and the appetite for food, which had been absent from the first, was at once excited. The pulse, which had been variable, but generally rapid and always weak since the development of fever, rapidly diminished in frequency and increased in strength."

III. We come now to Dr. Madden's account of the fever observed by him at Melbourne. "It is," he says, "an adynamic gastric or enteric fever, with many resemblances to the Edinburgh or relapsing fever: like it, relapses are very frequent, and the disease is often prolonged in consequence." "At the end of the summer of 1864," he writes, "this fever was very prevalent, and I treated several cases with *baptisia*."

10. A girl, æt. 7, convalescent in six days. "As a proof of the low condition produced by this zymotic poison, I may mention that two days after the fever left her she had copious and repeated epistaxis of dark blood, which, however, was speedily checked by *hamamelis*.

11. A boy, æt. 9. "Fever set in with headache, foul tongue, fœtid breath, tenderness of the abdomen, and all the indications of a severe attack." In eight days he was thoroughly convalescent. "In this case also there was epistaxis and oozing of dark blood from the nose for two or three days."

12, 13. Two cases of the usual fever, cured in three days.

14. "J. B., æt. 14, had a very severe and protracted attack of enteric fever, and was treated allopathically for nearly a month, when he became my patient. On my first taking charge of his case his condition was typhoid, with great tympanitic distension of the abdomen, black, fœtid diarrhœa, and low muttering delirium. In this state *baptisia* did but little, whereas *tereb.* 1 and *nitr. ac.* 1 acted well, and after a severe struggle for life he convalesced completely, and was going about as usual, when he became chilled by standing in the cold wind, and the fever returned severely with very rapid prostration. I at once gave *baptisia*, and in five days he was well."

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This case is especially valuable, as showing the genuinely typhoid condition into which this so-called "gastric" fever tends to run.

IV. In the same volume (XXIV) of the *British Journal of Homœopathy* which contains Dr. Madden's paper, we find, extracted from an American journal, the following account of seven contemporaneous cases of fever treated with *baptisia* by Dr. E. M. Hale.

15, 16, 17, 18, 19, 20, 21. "There was no epidemic in this city in the spring of 1866, until about the 1st of June, when it appeared in the form of a bilious remittent, with a tendency to a typhoid condition. The seven cases which came under my care in the first two weeks of June were all of nearly the same character. They were ushered in with a chill, followed by fever, with severe aching in the muscular portions of the body. Pulse ranged from 100 in a minute to 125, and was hard in the evening. The pain in the head was not acute, but a dull bruised ache. The whole body felt as if bruised and lame. Tongue dry, brown, or with a red, dry middle. Faintness on rising; giddiness and severe nausea; a good deal of thirst; bitter, nauseous taste in the mouth; sinking sensation in the stomach; diarrhœa with some pain and soreness in the bowels; the evacuations light yellow or brown, thin and watery. The odour of the breath was not very disagreeable, nor were the fæces very fœtid. The urine was usually very high-coloured and scanty.

"The first case that came under treatment was given *baptisia* and *bryonia* in alternation, and terminated with a profuse sweat on the fifth day.

"In the other cases the *baptisia* was given alone; a few drops of the first decimal dilution in half a glass of water, a dessert spoonful to be given every two hours. All these cases terminated on the fourth day of the fever, except one, and that on the third. In less than twenty-four hours after commencing the *baptisia* the dryness of the mouth and tongue, the sensation of soreness in the muscles, and the heat of the skin, were notably relieved.

"On making enquiries relative to the intensity and duration of the fever under allopathic treatment, I have ascertained that some cases were quite severe, and lasted fourteen and even twenty-one days. In two cases under homœopathic treatment, with *aconite* and *bryonia*, the fever continued twelve days."

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V. (Cases 22—50.) And now of Mr. Freeman's observations in the epidemic at Kendal in 1865. He treated 53 cases, and describes their usual course as follows:—

"A general malaise for several days, sometimes with a certain amount of shivering, was succeeded by a sense of weakness, by some aching in the head, back, and limbs, loss of appetite, with a furred tongue. There was weight over the eyes—the headache was generally frontal and weighty. There was a certain amount of delirious disturbance at night, varying from a mere uneasiness and inability to sleep, to delirium proper. These symptoms increased as the disease went on: the tongue became browner, flatulence and abdominal tenderness appeared, the strength rapidly diminished, the pulse became smaller and quicker, till the time for the usual change came round, when there was a cessation of all the symptoms except the exhaustion, which it required many days of careful nursing to remove."

In 29 of these cases *baptisia* was given at the outset, in the 1st centes. dilution. Mr. Freeman considers that in two of them the fever was aborted by it. He also writes: "The earlier cases in the series were treated with *rhus*, the results were quite satisfactory; but the provings of *baptisia* having attracted the writer's notice, it was given in a few cases, and removed so much of the distress of the fever, the delirium, the headache, the lassitude, the pain in back and limbs, that it seemed desirable to substitute it for *rhus*."

VI. (Case 51.) We have next Dr. Bayes's case, which seems to have much impressed him with the value of the remedy.

The symptoms were unmistakably those of true enteric fever. On the 14th day the typhoid condition was very pronounced, and the patient lay semi-comatose till the 17th, when she appeared to be dying. At this juncture *baptisia* was given, with immediate good result, and "the patient never looked behind her, but made a very rapid and capital recovery." One point especially noted is, that the urine, which was alkaline and very offensive, became acid and its odour natural within eighteen hours after taking the *baptisia*.

VII. (Cases 52 and 53.) Last of all, there are Dr. Edward Blake's fully and accurately reported cases. They were typically typhoid, and it cannot be said that in either

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baptisia proved abortive of the disease. But in the first at least it seems to have mitigated severity and shortened the normal duration, so conducing to a happy issue.

There have now passed before us 53 cases of fever, of which the majority were certainly examples of true typhoid, and the rest (excluding four examples of possible febricula, Cases 2, 3, 12, 13), presumably so, as I have argued. In all the effect of the medicine has been either to induce a speedy crisis, or materially to abate and curtail the disease. I am not acquainted with any other drug which can accomplish the former feat, so that there *baptisia* stands unique and alone. In the latter action it has several compeers, and its comparative value has yet to be ascertained. To this enquiry, and to the whole question, we must now bring a farther set of evidences, *viz.*, the experiments made to ascertain the pathogenetic action of the drug.

These show the following symptoms:

After taking during the day (Feb. 5th) four drops of the mother tincture, Dr. Douglass awoke in the night with a feeling as if the room were insufferably hot and close, hindering respiration. His pulse was about 90, full and soft. There was most uncomfortable burning heat of the whole surface, especially the face. The tongue was dry, and smarted and felt sore as if burnt. The heat compelled him to move to a cool part of the bed, and finally to rise and open a window, and bathe his face and hands. With these symptoms there was, he writes, "a peculiar feeling of the head, which is never felt except during the presence of fever, a sort of excitement of the brain, which is the preliminary to, or rather the beginning of delirium, which with me never fails to occur if fever continues and increases to any considerable intensity." He at length got to sleep again, but awoke the next morning with the same dry and burnt tongue.

The same symptoms recurred on the night of the 7th, after four more drops of the tincture. The oppression of breathing was still more marked, and felt quite congestive. Flushed face and dulness continued during the next day. Each night while awake he had painful intolerance of pressure as he lay, especially in the sacral region, obliging him at last to lie on his face.

On the 10th, after a dose of three drops taken in the afternoon, the same symptoms rapidly supervened. It is

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added that the head felt large, and the eyes were shining; the hands also felt large, and were tremulous.

He took no more medicine. The bowels had been constipated throughout the proving till the 12th, when they resumed their usual condition. He felt weak and tremulous, as if recovering from an illness, and was not himself again till the 15th.

The other provers had the same febrile symptoms, with hot and high-coloured urine. Dr. Rowley records vomiting and diarrhœa. Dr. Sapp, pain in the stomach, abdomen, and right hypochondrium, passing down to the right iliac region, also soreness in the region of the liver. Dr. Smith had diarrhœa, followed by constipation and hæmorrhoids; and constipation was present also in Dr. Hoyt.

Lastly, in a more ignoble prover, a cat poisoned by Dr. Burt, the small and large intestines were found congested and filled with bloody mucus.

From these symptoms two facts seem to stand forth with unmistakable clearness.

1st. *Baptisia* is capable of exciting true primary *pyrexia* in the human subject. This is no slight thing, for there are very few other drugs to which we can ascribe such power. When we have named *aconite, belladonna, cinchona* and *arsenic*, I imagine that we have come to the end of the list—the febrile symptoms of other members of the Materia Medica being secondary. And this pyrexia, in the case of *baptisia*, is exceedingly like that of the early period of typhoid. The soft and full, yet quickened pulse, the headache and tendency to delirium, the soreness all over, and intolerance of pressure when lying, are marked symptoms of this stage of the disease.

2nd. We have no evidence that *baptisia* affects Peyer's patches as they are affected in typhoid, nor even that it acts upon them at all as *arsenic* and *iodine*, and perhaps *mercury* and *turpentine* do. But it is certain that it produces congestion and catarrh of the intestinal mucous membrane, with abdominal tenderness and diarrhœa. Now this again, according to Aitken's description, is the condition present during the first week of typhoid. The Peyerian and solitary glands are, till the seventh day, involved merely in the general hyperæmia, but the latter then subsides, and they stand out alone.

We have, therefore, in *baptisia* a medicine precisely

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homœopathic to the first stage of typhoid fever, *i.e.*, to the period antecedent to the full development of the intestinal affection. There is nothing, I think, to render it inconceivable that, administered early and persistently in this period, it should abort the growth of the disease. There are two opportunities then afforded it for so doing. The *dothien-enterite*—as Bretonneau proposed to call the infarction of the intestinal glands—does not begin till about the fifth or sixth day of the fever, to which it is (as it were) secondary. Why should not its development be altogether prevented? Again, on the tenth day there is a natural tendency to resolution in the local affection. "The turgescence," writes Trousseau, "of the aggregate and solitary glands of Peyer, and of the mesenteric glands, begins to decrease, and goes on gradually subsiding up to the 14th day, at which date the affected glands are still a little swollen, but by the end of the third week resolution is complete, excepting that the mesenteric glands do not quite regain their normal condition till a short time later." Why should not *baptisia*, by abating the whole force of the malady, favour this tendency to resolution?*

Of such an action as the latter, we have an analogous instance in the case of *tartar emetic* in small-pox. It does not prevent the formation of the eruption: but it does strenuously promote its resolution in the papular or vesicular stage, so that the processes which end in pitting, and the concomitant secondary fever, are averted. But to completely abort the disease as suggested in the former alternative may be thought without precedent or probability. Yet we have a precisely corresponding action in that of *aconite*, when administered for the effects of a general chill. Fever may have been already set up, and a lung or other organ may be giving out signs of distress: but if actual exudation has not taken place, the whole process may be checked, and both general and local symptoms may speedily disappear, under the influence of this noble medicine. But it may be said,—there is here no specific process, no "essential" disease. The seed of a true morbid poison once sown in suitable soil, it is very doubtful whether its natural development can be checked. Well: let us look at cholera. This is surely as specific and essential a disease as typhoid. And yet "there is,"

* As it seems to have done in Case 8.

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as Dr. Russell writes, "the most perfect unanimity among all homœopathic practitioners as to the efficacy of *camphor* in curing the first stage of cholera"—*i.e.*, before the vomiting and purging have fully set in. Now the claim that some of us have raised for *baptisia* in typhoid is that *it is the aconite of its fever and the camphor of its enteric flux.* In the view of its entire homœopathicity to the stage defined, and of the general testimony in its favour of those who have tried it, I think we are not warranted in rejecting the claim. I fully admit that further and more precise experiments, both pathogenetic and therapeutic, are necessary to substantiate it. I maintain only that a case is made out for such experiments as our most hopeful outlook in the treatment of this dire disease.

But the analogy of *aconite* and *camphor* suggests a step farther. *Aconite* has some power, though short of an abortive one, over inflammatory fever in every stage. *Camphor* is, if Dr. Rubini is to be heard, in some degree an antidote to the cholera poison throughout. May it not be so with *baptisia?* Cases 1, 7, and 51, seem to shew that at no stage of continued fever is it without beneficial influence, and to lead to the inference that, unless other remedies are better indicated, the patient should always have the advantage of what it can do for him. It might be alternated in the more advanced period of the disease with the *arsenic*, the *mercury*, or the *turpentine* we should deem it right to give for the intestinal affection, just as we alternate *aconite* with the local specific in fully developed inflammations.

The evidence before us seems then to warrant the following conclusions:—

1. There is good reason for believing that in the common endemic continued fever of this country, of America, and of Australia, *baptisia* exerts true curative properties.

2. That there is as yet no ground for distinguishing such malady from the typhoid, intestinal, or dothinenteritic fevers of our present nosology.

3. That the pathogenetic action of *baptisia* shews it to be truly homœopathic to the pathological condition present during the first week of typhoid: and that it is accordingly possible that in this period of the disease it may be completely aborted by the drug, as *camphor* aborts cholera.

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4. That it is farther probable that *baptisia* promotes the tendency to resolution of the dothin-enteria which obtains about the tenth day of the fever, acting here like *tartar emetic* in checking the maturation of the pustules of variola.

5. That in every stage of the fever *baptisia* is worthy of due consideration as a remedy.

Pavilion Parade, Brighton.

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ON THE PHYSIOLOGICAL ACTION OF SERPENT-VENOM.

By J. Pyburn, M.D.

I have chosen this subject for consideration, because it is of practical interest; and if it even prove that I bring forth nothing new, which is exceedingly likely, yet I trust that the discussion on the question will settle the minds of any who may be in uncertainty. If it should appear that we have not reasons to warrant the use of these animal poisons, the sooner they are banished from our Materia Medica the better; but if, on the contrary, it can be shown that they produce symptoms common to many grave forms of disease, let us apply the law which is our boast and confidence, nothing doubting.

Snake-poisons have long enjoyed a reputation as curative agents; for centuries the natives of India have applied them in the treatment of various maladies. In Brazil a popular notion obtained, that the poison of the rattlesnake was a sure cure for the elephantiasis of the Greeks. And I believe that in the history of medicine of the most legitimate type in this country, the poison of the viper will be found as a notable remedy.

But it is not on such usage or on such popular notions that we would be justified in including it among our remedies. We possess a "touchstone" by which we can verify or correct first impressions, and reduce to an almost certainty the precise worth of the substance subjected to the test. Thus, given a history of cause and consequence, in a case of poisoning by any substance, noting its disease-producing power, we also note its power to cure disease.

It was therefore to be expected that the disciples of Hahnemann should be attracted by the stupendous effects of snake-poisons, and endeavour to utilise them in grappling with disease.

To Dr. Hering, of Philadelphia, I believe belongs the honour of first applying the test. Assisted by a band of men and women, willing to suffer some inconvenience for

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the sake of humanity, he furnished us with provings which to this day have been our guide.

Dr. Hempel has objected to these provings,* "that toxicologists have hitherto believed that the poison of serpents is digested by the gastric fluids, and cannot manifest any poisoning properties when introduced into the living organism through this channel;" and after quoting authorities supporting this opinion, the same writer goes on to say: "In spite of every effort to the contrary, the conviction has gradually forced itself on my mind, that the *pretended* pathogenesis of *lachesis*, which has emanated from Dr. Hering's otherwise meritorious and highly praiseworthy efforts, is a great delusion, and that with the exception of the poisonous effects,† with which this publication is abundantly mingled, the balance of the symptoms is unreliable."

Have not similar remarks been made by the enemies of homœopathy to all our provings? We have no reason to doubt the *honesty* of Hering and his coadjutors, and granted that the snake-poison is completely altered in its properties by the chemical action of the gastric juice, granted that it is no longer capable of manifesting poisonous deadly effects in the system, is it reasonable to say that it produces no effect, and that the almost interminable array of symptoms—I use Dr. Hempel's words—alleged to have been produced by *lachesis* poison, is the work of fancy rather than of actual observation?

I do not, however, admit the objection as proven: I am of opinion that the effect of the poison taken into the stomach would be much the same as when taken through the skin; but there would be a certain loss of power similar to that which has been observed in the two modes of giving morphia—a quarter of a grain of morphia will produce effects when given by injection subcutaneously, as great as a grain taken by the stomach. I am of opinion, then, that if we had the means of experiment—which we have not, in this favoured country—it would prove that a dose four times as large, taken by the stomach, would be as speedily fatal as a bite, and show similar symptoms.

The following case bears sufficiently on the point to justify its introduction. It is taken from the work recently

* Hempel's *Materia Medica*, Vol. II., page 336.
† The effects of the bite, I presume.

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published by Dr. Fayrer, styled *The Thanatophidia of India;* being a description of the venomous snakes of the Indian peninsula.

On July 16th, 1871, a woman (name and residence given) was aroused from a sound sleep at about 1 a.m., by a smarting sensation in the forefinger of her right hand, and thought something had bitten her. She had at the time her infant seven months old by her side, and her husband, with her other children, were sleeping in the same room, a little distance from her. She called out to her husband, and told him what had happened; but feeling very drowsy, and receiving no answer from her husband, who is excessively deaf, she fell into a slumber, and, while in that state, allowed her infant to take the breast. Soon after this she began to experience a painful sensation along her right arm, and a general restlessness of the whole body, and at the same time she observed that her child had likewise become very restless, and was foaming at the mouth. Alarmed at this, she got up and called her neighbours, who came immediately to her assistance. Nothing was done, however, in the way of treatment; there were certain chanting charms repeated over them—the usual mode in India among the natives. The consequence was both mother and child became rapidly worse and died, the mother four hours after she was bitten, and the child two hours after she had taken the mother's breast.

The narrator of this case* states that on the following day the bodies were sent to him for examination. The face in both cases was livid and swollen, and there was an issue of bloody froth from the mouth and nostrils. In the forefinger of the right hand of the mother was a distinct mark of a snake-bite. The body of the child was carefully inspected, but not the trace of a bite or any injury could be detected in any part of it. The conclusion come to was, that the mother had died of the snake-bite, and that the child was poisoned through her milk.

This case exemplifies the rapidity with which the fluids become affected by the immediate introduction of poison by the circulation. I think also that it proves that there is no very speedy alteration of the poison in the blood; and further, that the gastric juice of the infant, at least, does not so alter the poison as to render it innocuous.

* Mr. Shircore supplied the case to Dr. Fayrer.

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It is no part of my object to differentiate the effects of the various snake-poisons; but accepting the very generally believed opinion, that they are very much alike in their general appearance, composition, and effect, proceed to note their action, as related in a few cases selected from a great number, all going to prove the same.

The virulency of the poison seems to depend as much on the *quantity* injected as on any other condition; for after the snake has bitten several times in rapid succession, the supply of venom becomes exhausted, and although the reptile may be fierce, and bite, poisoning does not follow. The snake-poison produces its effects either by completely paralysing the nerve-centres, and thus causing rapid death, in some cases in fifteen minutes —or by partially paralysing them, and so inducing pathological conditions analogous to blood-poisoning. When it is remembered that thousands of persons, as Dr. Fayrer says, not less than twenty thousand, die annually from snake-bite, it is astonishing how few minutely described cases we meet with. I refer to that nicety of description which gives, not a mere outline sketch, but a well-developed picture of the process by which, step by step, the disease extinguishes vitality. This may be accounted for by the excitement, both of the sufferer and those called to his assistance, as well as the swift death that follows the injury. However, I have, I think, selected a series which will illustrate the main points to be observed.

The first effect of the poison seems to fall on the nerve-centres, and, I venture to suggest, mainly on the ganglionic system. The following case is recorded by Dr. Woodford, police surgeon at Calcutta:—*

N. C., a Bengalee Hindoo boy, aged 9 years, was bitten 2 a.m., July 29th, 1870. He was sleeping on the floor by the side of his father, whom he awoke, calling out, "I am bitten." The father states that the child screamed, was convulsed, and foamed at the mouth. He died in fifteen minutes. The body was examined at 9 a.m. of the 29th, or about seven hours after death. Rigor mortis was complete within an hour after death. The lungs were engorged with blood. Heart firm, both sides filled with liquid blood; great vessels natural. The abdominal viscera generally were congested. The blood

* Dr. Fayrer's, fol. 48.

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remained fluid; it did not coagulate. The brain substance was firm and natural.

Dr. Fayrer adds the following note:—The blood of this boy did not coagulate when set aside after death; under the microscope the red globules were crenated, and did not adhere to each other. There were no new cell forms. Forty drops of the blood were injected hypodermically into a fowl's thigh; the bird was perhaps somewhat sluggish after it, but no evil result followed.

G. M., a Brahmin, aged 45 years, was bitten by a cobra at 9.45 p.m. He had gone out of his house to pass water, and returned immediately, saying he had been bitten by a large snake. He became insensible in about ten minutes. He said he felt heated, but refused water before he became unconscious. He was dead in fifteen minutes.

The body was examined sixteen hours after death. The lungs were engorged with blood. The right side of the heart was distended with fluid blood. Left side full of fluid blood. Great vessels natural. The abdominal viscera were congested, and the stomach contained a quantity of milk. The blood was quite fluid, not coagulable. Brain vessels full of blood. Serum effused between convolutions. Rigor mortis occurred in an hour after death.

Were the deaths in these cases due to the alterations in the blood, or to the destruction of the nerve-power which controls the circulation of the blood? I think the latter. Remark the different ages of the victims. The dose of poison was no doubt large in the last case, as the snake was described as "large." The seizure calls to mind an attack of epilepsy. Is epilepsy not due to a temporary paralysis of the ganglionic nerve-centres? I incline to the opinion that it is. This by the way.

Several cases are related by a staff-surgeon* at Barrackpoor, in all of which speedy insensibility and foaming at the mouth occurred. He also mentions other cases that he had seen on the extreme north-western frontier; but there the cases were very different, being those in which a hæmorrhagic or purpuric oozing of blood from the skin and mucous membrane is the most marked symptom.

The next case that I will read (from the same source) is that of Captain S., who died of a snake-bite received whilst bathing, in May 1869.

* Dr. Verchere, op. cit.

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At 8 p.m., when in the water, he felt as if a crab had got hold of his leg; he shook off the assailant, and feeling no inconvenience from it, took no further notice of the circumstance. After the bath, Captain S. called at the house of a friend, where he stayed about an hour, amusing the children of the house by singing and playing the concertina. He seemed in high spirits, and expressed himself as feeling a sort of glow all over him, which was rather agreeable than otherwise; and his friend remarked that he had never seen Captain S. look so well. On reaching his vessel, about 10 o'clock, he complained of a sensation of thickening of the tongue, which rendered articulation difficult and indistinct; and gradually a feeling of stiffness all over the body came on. At about 11, when attempting to lie down, he felt as if suffocating. Rigidity of the muscles of the neck, arms and legs increasing, he took some brandy, which was retained only for a short time, and a doctor was sent for, who prescribed for him (not knowing he had been bitten). At about 4 a.m. of the next day, the symptoms mentioned, with sickness of the stomach, continuing, another prescription was given. At about 8 the same morning it was remarked by a Burman who had just then seen Captain S., that the symptoms were those of a bite from a "gyat" (poisonous water-snake). Captain S. then called to mind the fact of his having felt a bite when bathing in the river the previous night, and on examination the marks of the bite were discovered, two on either side of the tendo Achillis, near the ankle. The bites did not at all seem inflamed, nor did they give rise to any feeling of discomfort; in appearance they looked just like mosquito bites, but a little bigger. Slight rigidity and occasional spasms continued all day, with irritability of the stomach. Brandy was freely administered, with *cannabis ind.* mixture. Had a tolerable night (May 25th); a remission of all the symptoms under *morphia* 1 gr.; on the whole he seemed better; slight spasms on moving or attempting to sit up in bed. At about 6 p.m. was seized with violent spasms, which continued with slight remissions for an hour, when the patient expired, seventy-one hours after he received the bite. Cadaveric rigidity set in about three hours after death. No post mortem was made.

The slow procession of the symptoms in this case gave time for observation. It is evident that the earlier symp-

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toms were not inconsistent with those observable in disease of the brain at its base, where the most notable thing is the disturbed function of the glosso-pharyngeal, spinal accessory, and then later the pneumogastric nerves. Query—Were the "high spirits" of the patient, which seem to have attracted special attention, and the "glow all over, agreeable rather than otherwise," the effect of the bath or the poison? We know that some neurotics manifest their action by inducing excitement, they stimulate; by-and-bye they act as depressants—instance opium. Time will not permit the introduction of more cases *in extenso;* but I must give you a sketch of two or three more, where symptoms of hæmaturia, hæmoptysis, and other discharges of blood occurred.

A sailor,* one of the crew in H.M.S. *Algerine,* was handling a water-snake (seven feet six inches long, and six and a half inches in girth at its thickest), when it inflicted a wound resembling that caused by the point of a pin, on the index finger of the right hand. He declined having it fomented, as he had no fear of consequences, having, as he said, and no doubt believed, been bitten before by similar snakes. Half an hour after receiving the bite (at 8 a.m.) he made a good breakfast, dressed, and two hours later (10 a.m.) went on deck. After taking a few turns he was suddenly seized with vomiting, the matter ejected being of a dark brown colour, resembling coffee grounds, and of a very offensive odour. In a short time the pulse became small, variable, and intermitting—the pupils were dilated, but contracted under the stimulus of light. The *left side* of the face was slightly paralyzed. There was subsultus tendinum. The skin was covered with a cold, clammy perspiration. The countenance was anxious, and indicative of great distress. In consequence of spasmodic action of the muscles of the glottis, he breathed with great difficulty. He could not swallow medicine prepared for him. The integuments, from the bite to the wrist, were slightly swollen, and on the right side of the face they presented a mottled appearance of dark purple and livid colour. At 10.20 he had a bath, and whilst in it he swallowed a mixture which caused him to vomit a dark ropy fluid. Twenty minutes after coming out of the bath the spasmodic action of the muscles of the throat and neck became more severe, and the

* Op. cit.

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whole body assumed a purple colour. The breathing became very difficult, from the obstruction caused by the dark brown substance which came from the air-passages. By 11 o'clock he was in a state of coma; pupils contracted. At 11.20 he died; not quite four hours from the time he was bitten.

Does this case not suggest hydrophobia? Of course, more rapid in development and result.

From an account given by a sufferer from snake-bite, who recovered—having had, however, a very narrow escape—I transcribe a few of his observations.* He says:—

The effect was instantaneous; a sharp, hot pain shot up the whole length of the arm, along the course of the nerve affected, making me drop the creature faster than I had picked it up. I went directly to Dr. P.; he gave me brandy, and caused me to suck the wound. (The sufferer is understood not to increase the risk by this procedure; but no one else can be persuaded to perform the act.) This brought out a good deal of dark blue blood, from a puncture only large enough to be seen. Shortly after a violent throbbing headache came on; then sickness; the contents of my stomach were all turned out. All this occurred within three-quarters of an hour of the bite. Dr. P. brought a friend, another doctor, fearing the worst. Gradually the headache subsided, and drowsiness came on, but I was walked about, and dosed with strong hot punch. I must have consumed two bottles, or more, for a third was broached. The doctors remained with me all night. I did not, I think, wholly lose consciousness of my state at any time, but believe I talked a good deal more or less incoherently. Next morning arm much swollen, which did not go down for seven or eight days.

I will not occupy any more time in the recital of cases. Sufficient has been adduced to show that the virus produces a class of symptoms not infrequent from other causes. The cases prove also that dose, or quantity, has something to do with the violence of the symptoms manifested. Is it assuming too much to suppose that the quantity, or dose, may be so regulated as to be a manageable agent in the treatment of disease?

Dr. Fayrer, in speaking of the virus, says: "The

* Op. cit.

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poison may be diluted with water, or even ammonia or alcohol, without destroying its deadly properties. It may be kept for months or years, dried between slips of glass, and still retain its virulence. It is capable of absorption through delicate membranes, and therefore it cannot be safely applied to any mucous surfaces, though no doubt its virulence is much diminished in the endosmosis. It kills when introduced into the stomach, when put into the eye, or when applied to the peritoneum."

The elementary analysis of the poison has yet to be made.

The following statements appear not to harmonize:—*

"The flesh of an animal dead from snake-poison does not seem to be affected; animals and men eat it with impunity. The sweepers who attended my experiments always took away the poisoned fowls and ate them."

"The blood of an animal killed by snake-bite is itself poisonous; for if injected into another animal, it rapidly manifests its poisonous effects. I have transmitted the venom through a series of three animals, with fatal result."

With such an agent, capable of such easy dilution, and so easily kept, how is it that until lately we could not have it in quantity to enable us to administer it in lower attenuations than 6ths? I feel confident that the disappointment many have experienced in the use of these remedies is owing to the insufficiency of dose. This fault, thanks to Messrs. Thompson & Capper, is now likely to be obviated; besides, so terrible a plague are the poisonous snakes in India, that the Government offers pecuniary reward for their destruction. To obtain the reward the snakes must be produced alive, and magistrates of towns and villages are appointed to see them decapitated; and thus thousands are sacrificed, so that in larger towns this duty even becomes burdensome.

It would be an easy matter under these circumstances to obtain any quantity of the venom, by engaging some one with the requisite knowledge to collect the poison-bag and the secreting-gland; a plan preferable, I think, to importing the living snake.

I will bring this paper to a close by reading part of a "Note on the use of Snake-poison in Medicine, by the

* Op. cit.

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Kabirajes of Bengal," furnished by one of the most learned of that class for Dr. Fayrer's work.

After naming the snake, and describing the mode of obtaining, purifying, and preserving the poison, it proceeds to set forth its physiogical action thus:—" It is a warm irritant stimulant, a promoter of the virtues of other medicines, antispasmodic, digestive, a promoter of the action of the secreting organs;" and its therapeutical action thus:—" Used in the latter stage of low forms of fever, when other remedies fail, it accelerates the heart's action, and diffuses warmth over the general surface; clears the mind if coma supervene. In the collapsed state of cholera it is successfully used. It is employed in dysentery and some complicated diseases. Used in epilepsy arising from cold, relieving the patient from insensibility and forgetfulness symptomatic of that disease."

Other properties are ascribed to it, as antidoting the bite of a snake, &c. Moreover, says the writer, " snake-poison is the only medicine that can produce instantaneous effects on the whole system."

34, Prospect Street, Hull.

LONDON: W. DAVY AND SON, GILBERT STREET, W.

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TRANSACTIONS

OF THE

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PREFACE.

THE Annual Congress of medical men practising homœopathy in Great Britain was held at Leamington on September 11th, 1873.

The papers that were read are now republished in a separate form, for distribution to the members; the discussions upon them will be found in the October number of the *Monthly Homœopathic Review*.

It was decided by a large majority that the next Congress should be held in LONDON during the month of June, 1874, and that the precise day should be arranged by the Executive Committee, in concert with the Council of the British Homœopathic Society, so that the annual meeting of the latter may not be interfered with.

On the following page is given the list of officers elected for the direction of the next meeting.

(Signed) J. GIBBS BLAKE, C. P. COLLINS, } *Honorary Secretaries.*

November 1873.

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1874—LONDON.

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THE KIND OF ACTION OF DRUGS. THE ACTION OF SMALL DOSES. A LAW FOR THE DOSE.

By William Sharp, M.D., F.R.S.

"I wish you to get into the habit of contemplating the whole science of medicine under its simplest and plainest form."

Sir Thomas Watson.

A knowledge of the action of drugs is certainly one part of the science of medicine. Let us try to follow the advice of Sir Thomas Watson, and seek this knowledge in "its simplest and plainest form."

The use of drugs as remedies in disease is so ancient, that the history of its origin has faded away from the human memory: it is a subject which, on account of this antiquity, claims to be approached with respect. The use of such medicines is co-extensive with the families of mankind: the subject deserves, on this account, to be handled with care. Above all, the use of drugs as remedies is needed only during human suffering, and in man's last agony on earth: it demands, therefore, to be discussed with seriousness.

Let us, to-day, approach this subject with respect; let us handle it with care, and discuss it with seriousness; and let us hope that a blessing from God will rest upon our endeavours.

The first thought which arises in the mind is a question which cannot be asked without surprise: how is it that, after so many centuries of the use of drugs, the knowledge of the right manner of using them is yet to be sought for? What is the answer?

With reference to all knowledge men are ignorant from two conditions: in one of these knowledge is placed; in the other, men themselves. The condition in which knowledge is placed is, that much of it is shrouded in darkness. God is light, and there is no darkness in Him; but he has seen fit to surround much knowledge with a darkness which is impenetrable by the eyes of men.

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These are hidden things, whether corporeal, intellectual, or spiritual, which belong to God.

The condition in which men are placed is, that there are things upon which the light shines—whether the light of the material sun, the light of reason, or the light of revelation—but we do not see them because our eyes are blinded. This knowledge is attainable, but we are ignorant of it. These things belong to man, and he ought to know them. What his eyes can see, if he will use them aright, he may and ought to learn. Truth, then, is like the sun; God may hide it from us by placing clouds before its face; or it may shine brightly, and we may not be able to see it, because we are blind.

Now, in the department of medicine there is much which is hidden in darkness, and which we cannot know; but there is also much upon which the light shines, and it is our blindness alone which prevents us seeing it.

Let us try to use our eyes both of body and mind, for the power of seeing is strengthened by use. We have to exercise our bodily senses in observing what is presented to them; and we have to engage our minds in remembering, thinking about, and comparing the resemblances and contrasting the differences of the things we have observed.

Before we can so use our eyes much dust has to be removed. A great blinder of the eyes is prejudice, another is self-interest, another is indolence, another is pride, another is self-conceit; all these throw dust into the eyes, and prevent us seeing things upon which the light shines. Let us wipe away this dust. These are evil conditions which beset us all, like so many cuttle-fishes spirting out their inky juices, and preventing the mind seeing and feeding upon the objects provided for its nourishment and growth. Let us labour to get rid of these evil conditions, always remembering that it is a great mistake to try rather to penetrate into God's darkness than to remove our own blindness.

Some portions of the impenetrable darkness have been pointed out in former Essays. For instance, that the *efficient cause*, or the *essence* of the action of drugs cannot be known to us, has been argued. Again, the great evil of feigning *hypotheses* to explain this hidden thing has been insisted upon. And here, to prevent a misunderstanding, suffer me to say that this word "hypothesis" is used in two senses; in one sense it is useful to the pro-

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gress of science; in the other it is the bane of science. "Hypotheses" or conjectures are of great use to *suggest* further enquiries and experiments; to *explain* unknown things they are hurtful and obstructive.

We will now make an effort to open our eyes, and to look at the action of drugs in "its simplest and plainest forms."

To this end we will first notice briefly three propositions respecting the action of drugs which may be considered as proved, and, I think, as accepted by the assembly I have now the privilege of addressing. This, for the purpose of making the propositions which are to follow more easily understood.

I. *Homœopathy in the general.*

What are remedies? Poisons are remedies. "It has been heard of old time in the world, that poison is the antidote to poison." This is as the proposition was expressed three or four thousand years ago. In modern times, and in the language of Shakspeare, it is expressed in the words "In poison there is physic." This is startling. How contrary to anticipation, to *à priori* reasoning, that that which does harm in health should do good in sickness! The human mind would never have discovered this by ages spent in cogitation and reasoning. Our knowledge of it is wholly due to observation. It is a *fact*, not contrary to reason, but beyond its reach. Nevertheless, our knowledge of its certainty is such that, as a proposition, it is indisputable. BRODIE himself could not doubt it. *Arsenic* and *aconite*, *mercury* and *belladonna*, *copper* and *veratrum* are among the most deadly poisons we are acquainted with; and are they not also among the best remedies we possess? This lies before us as a plain fact of observation, admitting of no question—much less open to any denial.

But how have mankind acted upon and carried out in practice this great principle? Truly with many blunders. Enormous doses of powerful poisons have been given, under the guidance of false views, in all ages, to the grievous detriment of the sick. Even HIPPOCRATES himself sometimes killed his patients with *hellebore*. Men in modern times have been misled in the same manner. Mr. Annesley's magnificent quartos, *On the Diseases of*

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India, are filled with scruple doses of *calomel*. Dr. Rasori boasts of giving half-dram doses of *tartar-emetic* in pneumonia, and others follow in his wake.

Such abuses as these have tended to bring the use of medicines into discredit; this tendency has been much increased since the manifest failure of the ordinary treatment of disease has been placed besides the success of the small doses of homœopathy, and so the expectant or nothing-do method is becoming the prevailing one of our time.

The abuse of a thing is not a good argument against its use; moreover, that which is true in the general must be true in its particulars. If all poisons are remedies, then each poison is a remedy, and the question arises, a remedy for what?

After ages of blundering and guessing, or to speak with more professional propriety, after the reign of a succession of hypotheses, we are indebted to SAMUEL HAHNEMANN for giving us the first part of the true answer. This brings us to the second proposition.

II. *Homœopathy in the individual.*

Drugs, when taken in health in certain doses, produce symptoms which are peculiar to, and characteristic of, each drug; and when given in smaller doses in diseases having similar symptoms, they are the best remedies for those diseases. Homœopathy in the individual. This is Hahnemann's homœopathy—"similia similibus curantur."

Whoever will look with open eyes at the experiments which have been made on healthy and on sick persons, during the present century, will be constrained to admit that the detail of facts summed up in this second proposition makes a statement as undeniable and as indisputable as is the general fact expressed in the first proposition.

Does not *arsenic* in certain large doses produce inflammation of the stomach and bowels? In certain small doses it cures both. Does not *phosphorus* cause congestion and inflammation of the lungs and bowels? We see it in smaller doses cure these diseases in a surprising manner. Does not *opium* in certain doses bring on congestion of the veins of the brain—one form of apoplexy? We see it in certain small doses cure such congestion, and stave off attacks of apoplexy threatening to be fatal. Do not *cantharides* cause strangury? And was not Dr. Greenfield

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sent to Newgate by the President of the College of Physicians for curing strangury with these spanish flies? And this notwithstanding that Hippocrates, more than twenty centuries before, had said that "what causes a strangury will cure it?" And thus of all the rest, for it is so true of every known drug, that it may safely be predicated of every unknown drug. Therefore it is a true induction. Does not *veratrum* in one dose produce cholera, and in another cure it? Does not *belladonna* inflame the brain, eyes, and throat, and does it not cure similar inflammations every day? And *bryony* cause rheumatism, and cure it? And *ipecacuanha* sickness, and *rhubarb* diarrhœa, and cure them? But where shall we stop? There is no known exception to this rule. We might as well be asked whether we are now sitting in light or in darkness, as be asked if we can doubt this proposition of Hahnemann.

The proposition has, indeed, been denied, misrepresented, and ridiculed in an amazing manner, and with amazing perseverance; but it is true notwithstanding, and its reception by the medical profession as a body cannot be long deferred. The real difficulties which surround it, the substantial objections which may be urged against it, we will notice by-and-bye, and I think it will be our privilege to see them removed.

The various ways in which the discovery of the action and use of drugs may be sought for have so recently been studied in these Essays, they need not detain us now. Here we are agreed that the true method of making this discovery is, first, to observe the effects produced by drugs upon health, and then to observe their effects upon disease.

By pursuing this method Hahnemann was led to make the announcement of the dogma, "*similia similibus curantur*," which he explains in these words:—

"Take the medicines according to the symptoms careful and repeated observation has shown they produce in the healthy body, and administer them in every case of disease that presents a group of symptoms comprised in the array of symptoms the medicine to be employed is capable of producing on the healthy body; thus will you cure the disease surely and easily."*

This was the first modern statement of a guiding prin-

* *Lesser Writings*, page 587.

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ciple. The first view of the pole-star in medicine. Like the pole-star in the heavens, when first viewed by the naked eye it appeared to be but one star; moreover, as so seen it was nebulous, hazy, indistinct.

It has just now been remarked that there are difficulties which beset this proposition as stated by Hahnemann, and which are true objections to it.

The first of these is the haziness or indistinctness which surrounds it. This defect robs it of its brightness in a scientific point of view.

It is a two-fold defect, for this indistinctness or indefiniteness belongs to it in two directions. One of these is *the indefiniteness of its application*. Hahnemann made his principle applicable to an unlimited extent. For example, he imagined it to apply to the physical agencies of light, heat, electricity, and magnetism; again, to the moral feelings; and again, to the action of one disease upon another. These obscurities were fully discussed, and, it is hoped, cleared away in the early Essays of this investigation; and do not require to be noticed now beyond the remark that it seems to be proved in those Essays that the principle called homœopathy is limited in its application to the action of drugs.

The other direction of this defect lies in *the indefiniteness of similarity itself*. The term "like" or "similar" is too vague to satisfy scientific accuracy. In this sense also, as well as in the one just noticed, astronomers would call it nebulous, that is, wanting the defined lustre of a star. How is this defect to be removed?

When we have looked at the pole-star in the heavens through the telescope of the astronomer, we have seen that it is not a single but a double star. When we have looked at the principle of homœopathy through the telescope of time we see that it also is not single but double. *Similarity* still remains as belonging to the symptoms, but *identity* makes itself visible as belonging to the seat of the symptoms. The first star, though hazy, continues to shine as before, but the second star shines also, and is less nebulous, less hazy, less indistinct. And this is the third proposition:—

III. *Organopathy.*

The symptoms which each drug produces, when taken in health, are assignable to certain parts of the body as

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their *seat;* and they are remedies for the diseases of those parts. This is organopathy. The word is intended to express the identity of the seat of drug action and the seat of disease.

This proposition has been so fully stated and examined in former Essays that it does not need to be explained or vindicated now.

Like Hahnemann's proposition it has met with vigorous denial, opposition, and ridicule; and it has been declared to be an old and exploded doctrine of a German called Rademacher, notwithstanding that the epitome of his notions given in the *British Journal of Homœopathy* (1850, Vol. VIII. page 253) plainly contradicts this accusation. But at our Congress at York last year we were told by Dr. Drysdale that now "nobody doubts it."

Let me repeat. The principle of the founder of homœopathy, when limited as has been pointed out, remains in all its vigour. The comparison between the symptoms produced by large doses of drugs in health, and the symptoms presented to our observation in disease—all that Hahnemann meant to express by the phrase "*similia similibus curantur*," or by the word homœopathy, steadily abides.

And it may also be asserted that the identity of the seat of action of drugs, and of the seat of diseases for which they are remedies—which is the proposition meant to be expressed by the word organopathy—also remains unshaken by all that has been said against it.

We are now prepared to advance from these three propositions, which are known and received, to others with which we are less familiar.

IV. *The Kind of Action of Drugs.*

There is another difficulty or defect which belongs to Hahnemann's homœopathy which it is important that we should perceive clearly. It may be thus stated:—

The two portions of Hahnemann's method are the outside links of a chain of causes between which there are several unknown links. These two portions are the symptoms of diseases and the symptoms of drugs.

That drugs produce, when taken in sufficient doses in health, symptoms similar to those produced by other causes of disease, is a fact which does not require further proof at the present day. But the collocation or placing side

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by side of the symptoms of drugs and the symptoms of disease, so as to perceive their similarity, as is done by Hahnemann, is an empirical arrangement; and consequently the rule of practice that the drug having similar symptoms is the remedy for the similar disease, deduced from this arrangement, is an empirical law.

It is to be observed that these symptoms or detached facts, thus placed side by side, are separated from each other by a considerable distance, and that there must be several links of unobserved facts between them, and by which, as so many intervening causes, they must be joined together. The two outside links are recognised by us, but the connecting links are not yet seen distinctly. It is a duty to endeavour to discover these intermediate links.

The first has already been pointed out; it is the identity of the *seat* of action of drugs and diseases.

The second has now to be stated; it is the identity of the *kind* of action of drugs and diseases.

The third will be indicated by-and-bye.

Drugs are distinguished not only by their symptoms, and by the seat or origin of the symptoms, but also by the *kind* of action by which the symptoms are produced.

It has often been stated in these Essays that each cause of disease has an action peculiar to itself, by which it is characterised, and by which it is distinguished from others. Drugs are a cause of disease, and the same is true of them. Each drug has an action peculiar to itself, by which it is characterised, and by which it is distinguished from every other drug. We are now considering the action of drugs on healthy persons.

This difference consists partly in the action being on different organs of the body, and partly in its being of a different kind. The characteristic symptoms being the joint effect of these two differences.

Organopathy, the subject of the last proposition, deals with the different localities of the action. For the most part its details are sufficiently intelligible.

The kind of action is a more subtle existence, harder to identify and distinguish. Where does it begin?

The phenomena of life are a circle, and there is no visible beginning in a circle; but since we must begin our research somewhere, and since the blood is eminently the life, we may safely take the beginning of action to be in

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the blood. We will presume, therefore, that the first difference consists in a difference in the condition of the blood.

Each drug by its own presence produces this difference. This seems obvious. As each drug is a peculiar thing, its presence in the blood must be sufficient to constitute a peculiar condition of the blood. The living fluid, and the life of that living fluid, are affected by the presence of this foreign and peculiar thing.

The next stage is the action of this changed blood upon the living solids of some particular organ, or organs of the body; the part acted upon has been called the protoplasm, or, by Dr. Lionel Beale, the bioplasm.

This action on the bioplasm produces the objective symptoms—the symptoms which can be perceived by the physician.

Simultaneously with this action on the bioplasm impressions are made on the nerves of the part. These impressions are conveyed to the brain, and give rise to the subjective symptoms—the morbid sensations felt by the patient.

Suffer me to place these ideas before you in other words:—

"The blood is the life," and we may believe that the first contact between life and the causes of disease, including drugs, takes place in this living fluid. Here is the first meeting, the first impression, the first action, the first change from health, the first morbid phenomenon; the link in the chain which connects the exciting cause with all the successive phenomena of any disease whatever.

From this beginning arise inflammatory conditions of the arteries, congestions of the veins, obstructions in the glands, irregular contractions and paralysis of the muscles, derangements of the digestive and respiratory processes, morbid nutrition, altered structure, disease of all parts.

From this also spring all kinds of painful sensations and morbid feelings.

The condition of the blood, therefore, admits of as many alterations as there are kinds of substances which can enter into it and act upon its life.

If we take this condition of the blood as included in our present enquiry into the kind of action, there will be as many different kinds of action as there are causes of disease, or kinds of drugs.

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Of many of these kinds of action, and even of the existence of their causes, we should have no knowledge, but for the differences in their subsequent effects. For example: the causes of some contagious diseases are so subtle as to elude our observations. They act upon the same parts but give rise to very different symptoms; proving that the *kind* of action is different. They require different remedies, and the difference in the symptoms both points out the difference in the kind of action, and directs us in the choice of the remedy.

These remarks apply only to material causes of disease and material remedies. The mind has also power to disturb health, and even to destroy life. It may act upon the blood through the brain and nerves. It can derange the functions of any vital organ.

Thus it appears that drugs have many kinds of action. How are we to become acquainted with them?

Let us first have a clear perception that the action itself in its essence, that is, the link which joins the proximate cause to the proximate effect is one of the unknowable things; and let us not be ashamed to confess this. There are many things which we cannot *do:* such were the dreams of the alchemists, who tried in vain to turn lead into gold, and to discover the elixir of life; and there are many things which we cannot *know*, and we waste our time if we try to find them out. God's way of working in this sense is hidden from us.

Man's pride has made him labour to hide this unavoidable ignorance from himself by the invention of explanatory hypotheses or conjectures. Suffer me to repeat once more my protest against such hypotheses in medicine. They are the greatest barrier to progress which exists. Facts are grains of gold, and hypotheses are the mountains which cover them.

Another protest I ask leave to make against the use of words and phrases which are either void of meaning, or to which only undefined ideas can be attached. They are counterfeit coins of no intrinsic value, and serve only to deceive. True words are the current money of the mind. The word "catalysis" in chemistry is an example of a meaningless word: and in "The Nomenclature of Diseases," published by the authority of the Royal College of Physicians, the first division of "General Diseases" is

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a vague and undefined phrase from which nothing is learned. What is meant by calling "hooping-cough" and "mumps" "diseases of the general system?" There is nothing in nature but individuals; nothing in disease but disease of individual parts; nothing in the action of drugs but action upon one or more of these individual parts.

We admit, then, that the manner in which drugs act or the action itself, is beyond our power to discover. We remember that the action is always a local action, the antecedent cause of which, and its consequent effects, are phenomena we can observe, let us now enquire how this knowledge which is within our reach is to be obtained?

It is to be learned in the same manner as the knowledge of the seat where the action takes place, by experiments on healthy persons.

Experiments in health were made on a large scale first by Hahnemann. The results are recorded by him in a peculiar manner in his "Materia Medica" and "Chronic Diseases." They have been added to by similar experiments similarly arranged by others. These volumes have hitherto been the guide followed in the greatest part of homœopathic practice. They exhibit in great detail the symptoms caused by each drug. From these symptoms we may often learn both the seat and the kind of action of the drug to which the symptoms belong.

These experiments have commonly been made with large doses of drugs: doses sufficient to produce considerable and sometimes very great disturbance of health. The knowledge acquired is that of the action of large doses. Further experiments are required to be undertaken with the *intention* to discover the seat and the kind of action.

Nearly all experiments before Hahnemann, with such doses of drugs, were made upon the sick. The object of both sets of experiments, that is, both on the healthy and on the sick, is the discovery of the uses of drugs as remedies for disease. These two great labours of physicians resemble the tunnelling of Mont Cenis; an arduous work begun on opposite sides of the mountain; by the French on one side, and by the Italians on the other. Each had some miles to penetrate, and to persevere until it met the other. The earlier physicians laboured for centuries, guided by the notion that the action sought for must be of

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a *contrary kind* to that of the disease. The later ones have worked on till they have met the other, and the result is a surprise. It is the discovery that the compass which is to guide us in future is an action of the *same kind* as that of the disease.

The ideas I wish to communicate to you on this subject may, perhaps, be made clearer by giving some details relative to the action of particular drugs.

The heart. Aconite and digitalis. These plants have a powerful and well known action upon the heart. In the doses commonly experimented with in health, or to speak with more precision, in a certain range of doses, aconite quickens the movements of the heart, and digitalis diminishes them. The locality or seat of the action of these two drugs is the same. The kind of action of one is in the opposite direction to that of the other. They are antidotes to each other. Aconite is given as a remedy when the heart beats too quickly. Digitalis when it beats too slowly. The *kind* of action of the drug is the same as the *kind* of action of the disease for which it is a remedy.

The brain. Opium and Belladonna. These act with great power upon the brain. In the doses just described opium congests the veins and produces apoplexy; belladonna distends the arteries and causes inflammation. They are respectively given as remedies for the same kind of action in disease. The locality of the action of opium and belladonna is not so identical, nor is the kind of action so directly opposite as in the preceding example, but the former is sufficiently connected, and the latter is sufficiently contrary to make these two drugs to some extent antidotes to each other.

Spine and spinal nerves. Nux vomica and cocculus. These two drugs act powerfully upon the spinal system of nerves. Their contrasted kind of action deserves more careful study, but it appears, at present, to be in opposite directions. Nux vomica tending to produce spasms, and cocculus paralysis. Each is given as a remedy in cases of disease which correspond to its own kind of action, so far as that has been ascertained.

Throat. Mercury and belladonna. The difference in the kind of action of these drugs is very visible. The former causes ulceration, the latter inflammation of the mucous membrane of the throat. They are respectively

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given, every day, as remedies for the same kind of unhealthy condition.

Muscles. Copper and lead. These two metals have a strong action upon the muscular fibre. The former causing spasmodic contractions, the latter paralysis. They are successfully prescribed as remedies in the same kinds of morbid states. They act upon the same organs, but in opposite directions, and so are antidotes to each other.

Uterus. Secale and actæa. These have an action on the same organ but in opposite directions. The former producing contraction, the latter relaxation. They are to be prescribed accordingly.

Bladder. Cannabis and cantharis. It deserves careful experimentation to learn how far these are contraries to each other in their kind of action on the bladder.

Iris. Belladonna and Calabar bean. The former dilates the pupil, and the latter contracts it; and they are remedies accordingly.

Of course a large number of drugs have actions which are different from, though not contrary to each other in kind. The examples given are sufficient to explain distinctly what is meant by the *kind of action* of a drug.

By the discovery of two of the intermediate links, the outer ones upon which Hahnemann has hung the dogma "*similia similibus curantur*" have been brought into a clearer connection with each other. By this discovery the mystery in which the chain of causation was suspended is to some extent dissipated. The rule becomes less empirical, and is now not far removed from a true induction.

We have spoken of the principle of homœopathy as the pole-star of medicine; seen at first indistinctly it was hazy; on being looked at with more care a second star became visible; and having again been viewed attentively it is seen to consist of three bright stars instead of one. The astronomers would say that it is more satisfactorily defined.

Another simile has been used; the principle has been described as a basis or foundation. It is now three-sided, and it can support a three-sided column. On one of these sides "similarity of symptoms" was carved by Hahnemann; on the next is engraved "identity of seat of action;" and on the third is now written in legible characters "identity of kind of action."

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This chapter shall close with a few brief remarks upon *symptoms*, and the uses which may be made of them by thoughtful observation. They are of use—

1. To discover the seat of disease. For this purpose some symptoms are of much greater value than others.

In the same manner the seat of the action of each drug, if it is not actually visible, may be discovered from the symptoms. When a drug is prescribed as a remedy it must be able to act in the locality of the disease. In other words, the seat of the disease and the seat of the drug action, must, so far as we can ascertain, be the same.

2. To learn the kind of diseased action. For this also some symptoms are much more useful than others. When a drug is given as a remedy, its kind of action, so far as we can discover it, must be the same as the action of the disease. This second study of the symptoms serves to distinguish from each other the drugs which act upon the same organs.

These two uses are for the purposes of diagnosis and treatment.

3. To form an opinion of the probable result or termination of the illness. Again, some symptoms are much more of a guide than others. This is for prognosis. Sometimes the symptoms which at once decide the diagnosis are quite different from those which should guide the treatment, and both these may differ from the symptoms which determine our opinion of the probable result.

It is to be noted that the previous symptoms from the commencement of the illness, or what is called the history of the case, require attention as well as those which form the present condition of the patient.

We will now go in search of another of the yet undiscovered links in this wonderful chain of causes which join together the two outside links, the symptoms of drugs with the symptoms of diseases.

V. *The Action of Small Doses.*

My brethren, the time is come when the dangerous question of the small dose must again be taken up. Twenty years ago, as some of you may remember, I wrote an Essay upon it with deep interest. The novelty of the subject and its beauty charmed me, and I heartily desired that its contemplation might give as much pleasure to

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others as it did to myself. Since that time, whenever the purpose of continuing it has arisen, increasing difficulties have gathered round me, till I saw that the small dose could not be further dealt with, unless the principle of homœopathy could be placed upon a more stable foundation, and in a clearer light.

This task, I flatter myself, has now been accomplished.

The resumption of the pursuit after the truth of the small dose can, therefore, be no longer deferred. But it is a doleful and a thorny path, and in entering upon it I am reminded at the same moment, by its dolefulness of Æneas's trouble when Dido asked him to recount the disasters of the seige of Troy:—

"Infandum, Regina, jubes renovare dolorem;"

and by its prickles, of those sturdy thistles which are the national emblem of our northern neighbours, and which are surrounded by the appropriate motto:—

"Nemo me impune lacessit!"

Nevertheless, the time, which neither troubles nor prickles can postpone, is come, and the path must now be trodden.

An attentive perusal of the 14th and 15th of Dr. Dudgeon's 'Lectures on the theory and practice of Homœopathy' is sufficient to convince us that the question of dose is lost in the same dark labyrinth in the new school of medicine, that the question of remedy is in the old school. There are plenty of judgments given in each school on the difficulty which agitates it, and all these judgments profess to be based on experience. The results in both cases are a lively illustration of the truth of two portions of the first aphorism of Hippocrates, namely, that "experience is deceitful, and judgment difficult."*

Nothing can be more variable, nothing can be more contradictory than the many judgments given by Hahnemann himself, upon the doses of medicine. Sometimes he recommends substantial doses, little differing from common ones. Sometimes excessively small quantities, but these

* See also the leading article in the *Monthly Homœopathic Review* for August, 1871 (Vol. XV. page 449) by Dr. Pope; which contains as good a summary of the present state of knowledge and opinion on the question of dose as could be given.

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varying with each drug, with the nature of each disease, and with the susceptibilities of each patient. At one time he advocates the use of the smallest doses in acute disease, and larger ones in chronic cases; and he finally closes the subject by fixing upon the 30th dilution as the proper dose of every drug and for every disease. He laid down this law, but he did not himself follow it; he laid it down to bind others, but they have never been bound by it. The changes in Hahnemann's teaching on this and on other important points of medical doctrine and practice have drawn from Dr. Dudgeon the following damaging reflection:—

"How curious it is to observe that Hahnemann continually contradicts himself on almost every point of his doctrines and practice; and still more curious it is to notice that the contradiction is generally side by side with the opposite statement."*

There are no doses which Hahnemann has not at one time adopted, and at another abandoned. So that, in truth, the question is left by him in the greatest confusion. He sometimes used appreciable doses, and he introduced the use of indescribably small doses, but he discovered no clue by which we might be guided in this labyrinth.

Nor are the judgments pronounced by succeeding writers in the new school on the doses of medicines in any degree less contradictory.

Dr. Rau says: "The best guide is the maxim that the susceptibility of the organism for homogeneous irritation is in the direct ratio of the violence of the disease. Thus the more violent and acute the disease, the smaller must be the dose of the remedy, and the larger must it be, the longer the disease has lasted and the more chronic its character is."†

On the contrary, Dr. Trinks says, the maxims that experience has determined are—

"1. Acute diseases require the lower (stronger) dilutions.

"2. Chronic diseases higher (smaller), and often the highest (smallest) attenuations.

"3. There are many exceptions to this."

Dr. Dudgeon himself seems to adopt this view. He says:—

* Dr. Dudgeon's *Lectures*, page 406.

† *Ibid*, page 406.

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"Though we are still considerably in the dark with respect to the suitable dose of a remedy, I think this darkness has been partially dispelled by the experience of so many homœopathic practitioners extended over so many years. This experience seems to show that the more material doses, or the lower dilutions of medicines, ... are best adapted to diseases of rapid course and considerable violence; and that the higher dilutions are better adapted to diseases of a more chronic character; but there are many exceptions to this."

Dr. Geddes Scott in like manner argues that the potency to be chosen is determined by the character of the disease.

There are many other maxims and rules given by other writers which would occupy too much time were I to quote them.

Of these maxims and rules I beg you to observe two things:—

First. The authors of them admit that there are many exceptions to them; so that they can have no claim to be received as natural laws. They resemble the rules of grammar, not the laws by which God governs His works.

Secondly. They are derived from the same barren source from which the old schools of medicine have, in all ages, attempted to derive rules for the choice of remedies—namely, the consideration of the disease and of the patient, *not of the properties of the drug*. Now, Hahnemann's law of homœopathy was discovered by forsaking this path, and following another. He turned his attention to the action of drugs in health. It is surprising that homœopathists should have been beguiled into following again the old course, which had so signally failed, and should have expected to find rules for the dose in this way.

The knowledge we desire, and which we are now in search of, is the knowledge of the action on disease of small doses of drugs. How is it to be obtained?

It is said on all sides that the only way of learning the action of these doses, and how to choose the best, is the way of *experience*. But experience without a guide is a ship without a compass. Blind experience teaches little. It is always justifying the condemnation pronounced upon it by Hippocrates, and proving itself deceitful. The

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method of experience has failed. A *guide* for experience is needed; how is it to be found? Not by experiments on disease itself. These experiments have been made for many years, and by many physicians, and the guide has not been found.

Attention to Lord Bacon's Sixth Aphorism will help us:—

"It would be madness to suppose that things which have never yet been performed, can be performed without employing some hitherto untried means."

What means are there yet untried which we may adopt with renewed hope of success? For several years I have urged upon my colleagues the necessity of learning the action of different doses in the manner that Hahnemann discovered the action of different drugs. By experiments with them in health.

As far as I am aware no one has yet taken up this work. I shall, therefore, now endeavour to recommend it again, not by showing its necessity, but by beginning to do it.

The action of large doses—that is, of poisonous and of ordinary medicinal doses—has been made known to us, to a great extent, by cases of poisoning, and by provings, or voluntary experiments with them, in health.

The action of small doses, *as distinct from that of large ones*, has not been studied by separate provings of them in health. This knowledge is demanded, and it is the search for it which I have attempted to begin.

Experiments with small doses.

These experiments have been carried on for some time during every opportunity of leisure. They have embraced a considerable variety of drugs; among them these:—

Aconite.
Digitalis.
Phosphorus.
Spigelia.
Opium.
Veratrum.
Mercury.
Tartar emetic.

Most of the experiments have been several times re-

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peated. The doses experimented with have not embraced quantities smaller than the first centesimal dilution or trituration: that is, the hundredth part of a drop of the sap of a plant, or the hundredth part of a grain of a mineral. No opinion respecting the action of smaller doses than these is given; they remain to be investigated by others. Nearly all the experiments were made upon myself, and under conditions as nearly uniform as was possible; this has appeared to me a great advantage. It may be remarked that I am sensitive to the action of small doses, having taken none other for three and twenty years; it is not presumed, therefore, that *exactly the same doses* will be followed in every one by the same effects; but it is believed that these experiments have been so carefully made that they are typical, and that the same results will follow in all other experiments, made with equal care, *within a certain range* of these doses.

This is not the opportunity to give the details of these experiments, but I am happy to be able to state some of the conclusions which may be drawn from them, and to give some brief examples.

And, first, of individual drugs:—

Aconite.

Aconite seems to have at least four different kinds of action upon the heart, which are dependent upon the dose.

First action. A dose of one or of two drops of the first centesimal dilution first quickens the heart's action for a short time (one, two, or three minutes), then retards it.

e.g., Oct. 19, 1872, one or two drops of this tincture taken in separate experiments, when the pulse was 80, in one minute raised it to 82, in two minutes to 84 or 86, then, after the third minute, it fell to 82, and after the fourth minute to 78 or 76.

Second action. One or two drops of the sap of the plant (two or four drops of the "mother tincture") quickens the heart's action, and no retardation follows.

e.g., Dec. 3, 1872, my pulse at 72 was raised in three minutes to 75, in five minutes to 78; it fell in ten minutes to 76, in fifteen minutes to 73, and in twenty minutes from the commencement of the experiment to 72, at which it remained.

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Third action. We know from many published cases that larger doses than these (several, or many drops of the tincture) first depress the heart's action for a short time, then quicken it very much.

Fourth action. From similar sources we learn that still larger doses (some solid portions of the plant itself) destroy life during the first depressing action. Mr. Gordon, of Dingwall, died in this manner.

These four kinds of action of aconite may be reduced to two in their tendencies. First, the action of small doses; the tendency of these is to quicken and then to retard the pulse. Second, the action of larger doses; the tendency of these is to retard or depress, and then to quicken the heart's beats. When the experiments fail to produce these consecutive effects there is some impediment in the way.

Aconite, then, in small doses, quickens the heart's action for a brief period before it retards it. This has probably been observed for the first time. The observation surprised me.

It very soon makes the pulse slower. This we are all familiar with in the use of aconite as a remedy in inflammatory fever. I lately saw a pulse of 130 brought down to 40 in about three days, by repeated doses of this first dilution of aconite.

The range of these small doses having this action on me lies between the first dilution and the one-fifth part of a drop of the mother tincture: that is, from the hundredth to the tenth part of a drop of the sap. It is probable that in others it may lie between this tenth part and a whole drop.

Of all these varied actions of aconite upon the heart the only truly curative influence is the second action of the small dose. It follows that the only legitimate use of aconite is in doses which produce this retarded motion of the heart.

Digitalis.

Hahnemann says that aconite and digitalis are "analogous remedies." It is true that the locality of their action—the heart—is the same, but their kind of action on this organ is in opposite directions.

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Oct. 21, 1872. 7.30 A.M.
Pulse 80.
One drop of Tinct. Digitalis 1 (cent.), taken

1st minute, 80.
2nd „ 80.
3rd „ 80.
4th „ 80.
5th „ 80.

6th minute, 80, second drop taken.
7th „ 76.
8th „ 76.
10th „ 78.
12th „ 76.
14th „ 78.
16th „ 80.
20th „ 80.

21st minute, 80, third drop taken.
22nd „ 76.
23rd „ 76.
25th „ 76.
27th „ 78.
29th „ 78.
30th „ 80.
32nd „ 83.
34th „ 84.
36th „ 85.
40th „ 84.
63rd „ 80.

It will be noticed that this action is the reverse of that of similar doses of aconite.

8.30 A.M., an hour after commencing the proving, there was a very decided action on the kidneys, shown in increased secretion.

A second kind of action is produced by doses of digitalis a hundred times larger than these:—

Dec. 2, 1872. 12.30 P.M.
Pulse 64.
Two drops of Tinct. Digitalis ♀ taken (equal to one drop of the sap of the plant.)

5th minute, 68.
10th „ 72.

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15th „ 68.
50th „ 64.
At which it remained.

Three and four times this quantity produces a third kind of action :—

June 11, 1873. 7.35 A.M.
Pulse 66.
Six drops of Tinct. Digitalis ♀ taken.

2nd minute, 68.
4th „ 67.
6th „ 66.
8th „ 67.
10th „ 68.
15th „ 66.
20th „ 63.
25th „ 64.

June 16, 1873. 7.50 P.M.
Pulse 72.
Eight drops of Tinct. Dig. ♀ taken.

2nd minute, 73.
4th „ 74.
6th „ 76.
10th „ 74.
40th „ 72.
60th „ 66.
80th „ 64.
100th „ 72.

The result of these and similar experiments with digitalis, both in health and sickness, is the conviction that the curative action of this plant is the second action of the first centesimal dilution. This is the power which it possesses of increasing the contractile force of the heart—both in frequency and strength. It is the direct opposite of aconite. In old language aconite is a febrifuge; digitalis a tonic. It is my belief that these drugs are never legitimately employed as remedies in affections of the heart except in these two opposite capacities.

It is further worthy of notice, in relation to these two plants, that they both have a powerful action upon the kidneys, and that they both act upon this organ in the same direction. A certain range of doses of aconite and

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of digitalis increase the secretion of urine, and a certain range of larger doses of both cause suppression of it.

Your time must not be occupied with *cases*, but one may be given as an example :—

Case.—Mrs. S., a stout lady, of 58, has frequently suffered from disturbance of the heart, the first occasion of which was an attack of rheumatic fever, and which is characterised by a feeble and intermitting pulse, a sense, not of acute pain, but of great discomfort about the heart, especially when walking, and general weakness. This state of things has been repeatedly removed in a few days by digitalis. The dose is one drop of the first centesimal dilution—the hundredth part of a drop of the sap—three times a day. The intermission in the beats of the heart ceases soon after beginning to take the medicine, and the pulse becomes slower and stronger. The benefit derived by the patient is always very striking to herself.

Let it be remembered that aconite and digitalis are opposites in their action on the heart. They are, in fact, antidotes to each other. The action of the two is as opposed as the action of belladonna and Calabar bean upon the pupil. If we would use them aright as remedies we must give aconite to diminish its power and digitalis to increase it.

Phosphorus.

We are indebted to Hahnemann for teaching us how to make the doses of this poisonous substance small enough to be used with safety and success as a remedy. We learn that in the doses he used in his provings of it, its action on the heart is powerful. He gives these symptoms :—

"Fever with small, hard, quick pulse. Accelerated circulation of the blood. Throbbing of the carotids. Pulse quick and full; or, pulse quick and faint."

On the principle of *similia*, &c., therefore, it is used in inflammation and inflammatory fever.

The following is a careful proving of a small dose :—

Oct. 19, 1872. 8 P.M. Pulse 74.
One drop of Tinct. Phosp. 1 (cent.) taken.

1st minute, 76.
2nd „ 76.
3rd „ 74.

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4th „ 72.
5th „ 72.
6th „ 72.
7th „ 70.
8th „ 70.
10th „ 72.
20th „ 74.
Which continued.

8.30. Pulse 74.
Second drop taken.
1st minute, 72.
2nd „ 72.
3rd „ 72.
4th „ 70.
5th „ 70.
6th „ 70.
7th „ 70.
8th „ 72.
10th „ 72.
20th „ 70.
Which continued some time longer.

From these experiments it appears that phosphorus acts upon the heart in small doses in the same manner that aconite does in similar doses. So that, to this extent, its use in inflammation and inflammatory fever is justified.

Spigelia.

It has often been remarked with regret that Hahnemann does not tell us the doses he experimented with in his provings of drugs in health. We may, however, form some idea of the doses of spigelia used by him from his preface to the proving of this plant. He says:—

"The provings ought to be instituted with the greatest care, inasmuch as even *sixty, eighty,* or a *hundred drops* of the tincture produce powerful effects in even robust and very healthy persons."

From such experiments as these we learn that spigelia acts strongly upon the heart. The pulse is said to be irregular, now slow, now quick; it falls from 72 to 54. Palpitation and unusually strong beatings of the heart, &c.

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The following proving shows very accurately the action of a small dose of spigelia upon the heart :—

Oct. 21, 1872. Evening 7.20.
Pulse 77.

Tinct. Spig. 1 (cent.). One drop taken in 2 dr. of water.

1st minute, 76.
4th „ 75.
8th „ 78.
10th „ 77.
12th „ 77.
20th „ 77.

7.40. Pulse 77. A second drop taken.

2nd minute, 79.
5th „ 78.
10th „ 78.
15th „ 78.
20th „ 78.

8.0 o'clock. Pulse 78. A third drop taken.

2nd	minute,	80.			
5th	„	80.	Respirations	10	and deep.
10th	„	82.	„	8	„
15th	„	84.	„	6	„
20th	„	80.	„	8	„
25th	„	79.	„	10	„
30th	„	79.	„	14	no longer deep.
35th	„	78.	„	16	„
60th	„	77.	„	17	natural.

Taken by surprise by the slow and deep breathing.

It will be observed that this action of spigelia on the heart is the opposite of the action of phosphorus and aconite, and that it corresponds with that of digitalis. Its use as a remedy is indicated as one similar to the use of digitalis—to strengthen the action of a feeble heart.

It will also be observed that a new element is introduced in this proving, namely, the function of respiration.

Let us linger over this a few moments. The effect, after the third dose of spigelia, was very remarkable. It took me quite by surprise. The breathings became so slow and so deep that it was in fact a process of slow and deep sighing. This ought to be remembered when we

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meet with a similar condition of respiration in a patient, especially in connection with a feeble heart.

It may be objected that the respiratory function is not so entirely removed from voluntary influence as the heart's action is, and that this slow breathing may have been, partly at least, my own doing. To obviate this objection experiments were made (on Nov. 22, 1872) on *voluntary* slow and quick breathing.

It was found that it is possible to get over a minute with *three* breathings; or to make above *thirty*. But the effort required to accomplish either of these feats is considerable; so that a conscientious experimenter can have no difficulty in avoiding this cause of error. He can distinguish very well involuntary slow or quick breathing from any such efforts of his own.

It should be remarked that in these provings of small doses of drugs, intimations that other organs were slightly affected were occasionally given; for instance, while taking the spigelia, a shoot of pain through the forehead was felt. But these are omitted to prevent complications and distractions of thought.

Opium.

This is considered the most valuable of all drugs by the ordinary practitioner, and he gives it every day as a sedative and as an astringent.

It is well known that large doses of opium, or its tincture laudanum, do possess these two kinds of action.

It is also well known that comparatively small doses (from the eighth to a quarter of a grain or more) have a contrary action: that is, a stimulant one upon the circulation and the nervous system. This action, though known, is not much taken advantage of by physicians. It is sought for by a large multitude of people who take opium themselves.

It is not yet known that such small doses as are under investigation in this essay also possess the power of increasing the heart's action, and of enlivening the brain.

I have tried many experiments with doses from the hundredth part of a drop (the first dilution) to two drops of the tincture of opium, and in every instance the pulse has been quickened, and sometimes rendered sensibly fuller, in two or three minutes, *e.g.*, from 66 to 68 and from 70 to 76. After a longer or shorter interval, *e.g.*, in

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from five minutes to an hour, determined by the dose, the beats return to what they were at the beginning, and sometimes afterwards descend a little below this. The manifest effect, in all these experiments, is increase of frequency, and often of fulness also.

The contrary of the small dose to the astringent effect of the large one is known only to homœopathists; but they do not always avail themselves of this knowledge as they might do. This is really a serious neglect, and in order to direct more attention to this valuable action of opium, I think it right to put on record the following case:—

Case.—An officer in the army, holding a high position in India, was in England in 1872. He was in good health, but inconvenienced by constipated bowels. He placed himself under the care of a very popular homœopathic physician in London, who prescribed for him for five months. Among the remedies prescribed were—nitric acid, 1 dec. ʒ iij. in aq. ℥ iv.: a teaspoonful in water twice a day; bryonia, ♍ ʒ ij. seven drops in water night and morning; podophyllum, gr. iij. sacch. lac. gr. vj. six powders, one at bedtime; tinct. hydrastis, ♍ ʒ ij. 7 or 8 drops in water night and morning; acid phosphor. Ph. B. 10 drops in water, twice a day; tinct. chin. nitr. 1 dec. ʒ iv. aq. ad ℥ viij., a tablespoonful in water 2 or 3 times a day; tinct. nucis vom. ♍ gutt. xij.; sacc. lac. gr. xij. in a powder for an injection. Nux v. 1 dec. for two doses, &c. All this was without any benefit. He came down to Rugby in November. I gave him tinct. opii 1 (first centesimal dilution), part of a drop night and morning as long as required, but to be omitted when the bowels act. In March, 1873, he came again, and reported that the medicine for the bowels had answered perfectly; he had been two months without taking any; and was quite well. A degree of torpor or sluggishness of the bowels had, in fact, been his only ailment, and the small doses of opium had removed this.

Veratrum.

That white hellebore produces excessive diarrhœa has been well known since the days of Hippocrates. This is the action of the doses commonly given.

I have found that one drop of the first dilution of the tincture (that is, the hundredth part of the drop of the

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sap of the plant), taken night and morning, will produce constipation in four or five days. The person upon whom this was proved was a healthy young man.

Mercury.

In the same manner it is very well known that preparations of mercury are given every day to produce relaxation of the bowels. Half a grain of the first or second trituration of metallic mercury taken night and morning for some time will constipate the bowels.

Tartar emetic.

The action of this salt of antimony, as learned from cases of poisoning and from the provings hitherto made, appears to be on

The stomach and bowels,
The skin,
The kidneys,
The heart,
The lungs.

The organs which commonly escape its action are

The brain and spinal nerves,
The voluntary muscles,
The bones and joints.

Not only the kind of action, but also the organs selected to be acted upon, depends upon the dose. A grain will act on the stomach and cause vomiting; a quarter of a grain will act on the bowels and purge; one-sixteenth of a grain repeated a few times will act on the skin, and produce perspiration. Some doses will bring out an eruption on the skin very closely resembling the eruption of small-pox. The question to be answered now is this: What will the hundredth part of a grain do?

Nov. 4, 1872. 7.0 P.M. Ant. Tart. 1. One drop in ʒ j. of water.

Pulse, 72		Breathings, 18.	
2nd min.	70	4th min.	20.
5th „	72	8th „	18.
10th „	67	12th „	14.
15th „	72	18th „	13.
20th „	71	24th „	12.
25th „	74	28th „	10.

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	30th	„	68	{ Deep, uncomfortable sighings.			
Quickened by the deep breathing.	35th	„	70	....	34th	„	8.
	40th	„	72	....	38th	„	7.
	50th	„	77	....	42nd	„	6.
	60th	„	66	....	52nd	„	12.
					60th	„	18 } natu-
	120th	„	66	....			18 } ral.

In this experiment the pulse, it will be seen, was slightly decreased in frequency, but the principle action was upon the respiratory process, which equalled that of spigelia.

This seems to be the place to notice the apparent *want of effect* of very large doses of some drugs. Tartar emetic, though so active a poison in doses of a grain or two, has been given in half-dram doses with little result. Tincture of digitalis has been given in ounce doses "with much less effect than might be imagined." "MM. Trousseau and Pidoux tell us that they have taken half an ounce of good asafœtida at one dose, with no other effect than that of altering the odour of their secretions, by which they were kept for two days in an infected atmosphere possessing a more horrible degree of fetidity than even asafœtida itself!" I know nothing which can be inferred from these remarkable facts.

These examples of the proving of small doses must suffice, but before proceeding to more general conclusions it may be useful to repeat the results obtained relative to the pulse and the breathing.

The Pulse.

Aconite first quickens, then retards the pulse.
Digitalis reverses this order.
Phosphorus acts as aconite does.
Spigelia reverses this also.
Bovista acts too slightly to detect any order.
Lead does the same.
Oleander first quickens the pulse.

The Respiration.

Spigelia reduced the breathings from 17 to 6.
Bovista raised them from 17 to 22.

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Lead first depressed them from 18 to 15, then raised them to 20. A second dose reduced them to 11, then quickened them to 23.

Oleander first quickened, then lowered them, reversing the action of lead.

And let me make one observation on these facts. We find a great deal said in works on medicine on the sympathy of the different organs of the body with each other: as of the brain and stomach, the heart and lungs, the skin and kidneys, &c. And all this is true, and of great interest. But the other side of the subject is rather overlooked; that is the independent action even of organs most closely connected with each other, as in the above instances of the heart and lungs.

The action upon these organs, whether of disease or of drugs, is not necessarily of a uniform or equal character. The pulse may be either quickened or made slower, and not the breathing, or *vice versâ*. In a case of pneumonia I saw a short time ago, on one day the pulse was 130 and the breathings 30; the next day the pulse had fallen to 120 and the breathings had risen to 36. Still greater differences were observable in the provings just recorded, showing the same independency.

The following general conclusions, or inductions, may, I think, be safely drawn from the experiments which have been made with small doses:—

1. The kind of action of drugs varies with the dose.
2. This variation, in a certain range of large doses, amounts to opposition to the kind of action of another range of small doses.
3. The direction of the range of large doses is the same as that of the diseases for which, in small doses, they are remedies.
4. The direction of the range of small doses is in opposition to that of the diseases which they cure.
5. This opposite tendency is shown in health. Its cause, therefore, is not a difference in the state of the organ arising from disease, but in the quantity of the drug.
6. The varying conditions of disease have their influence on the action of drugs, but the effects of this influence are not at present under investigation.

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We are now prepared to consider how the information, so carefully obtained, bears upon our position as homœopathists.

Those who have honoured the early Essays of this investigation with a perusal, will remember that attention was asked to the vague and indefinite character of Hahnemann's law of homœopathy. He, in fact, as we all know, extended its application on all sides. This vagueness is one reason why the teaching of Hahnemann has been so distasteful and unsatisfactory to the medical profession.

It will be remembered that it has been carefully shown in these Essays that the law should be limited to the action of drugs.

The next step was an endeavour to prove that symptoms, whether of diseases or of drugs, must be traced to their seat: the organs which are the seat of the action, both of the disease and of the remedy, being the necessary substratum in which the action takes place.

You will see that it is now my duty to point out another limitation.

The rule of similarity of action, as manifested by the symptoms of diseases and of drugs—the law of homœopathy—must be confined to the action of comparatively *large doses* of drugs.

Taken in this restricted sense—restricted not only to drugs, but to large doses of drugs, and to their action in health—the law or rule, we may venture to say, is irrefragable. It is a natural truth.

But it is a half-truth. This is another reason why it has not met with acceptance. Galileo's telescope consisted of two glasses: one had been looked through a long time by itself, but it was not till the other was found, and the two were placed so that both could be looked through together, that Jupiter's moons were seen.

Hahnemann's half-truth is the *similarity* of the action of large doses to the symptoms of diseases which small doses can cure.

The other half-truth now added is the *contrariety* of the action of small doses to the action of large doses, and consequently to the action of the diseases they are remedies for.

These two half-truths now put together make the treatment of disease as visible as Galileo's two glasses did Jupiter's moons. Those only who refuse to look through the glasses can fail to see either the one or the other.

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A harmony in music is concord, the agreement of one note with another. A harmony in science, according to Lord Bacon, is the just adaptation of one part to another. The two parts of this subject, now joined together, make one harmonious whole.

You know that any two colours which, when combined, produce white light are called complementary colours. These two half-truths—the action of large doses and the opposite action of small ones—are the two complementary colours: their juxtaposition makes white light.

Hahnemann's homœopathy, I have said, is a half-truth. It is my privilege to-day to announce to you the other half-truth. I presume to think that the two halves make the whole truth, and that this will meet with your hearty acceptance.

The other half-truth then is this:—the action of small doses of drugs is in the opposite direction to the action of large doses. Therefore the law of Hahnemann, *similia similibus curantur*, remains true when limited to large doses; and the law of Galen, *contraria contrariis curantur*, is true when limited to the action of small doses; not true in Galen's sense, nor in any former sense put upon the phrase, but in a new sense, a sense which expresses a fact, and not a speculation.

VI. *A Law or Rule for the Dose.*

Some writers on homœopathy, as you know, have declared that a law for the dose is an impossibility. Such alarming prognostications lose a good deal of their gravity by repetition. I remember Dr. Lardner writing a pamphlet to prove that it was impossible for ships to cross the Atlantic by steam; and his pamphlet appeared just as a steamship reached America. Others have said that though a law may exist, the complexities surrounding it are so great, there is no probability that it can be discovered. Others again assert that "there is no necessary connection between the dose and the homœopathic law, the one exists quite independently of the other."*

On the other hand many laws have been suggested. I will not detain you by describing them; you will find them in Dr. Dudgeon's *Lectures*. They all have one condemning defect—they are hypothetical or speculative, and

* Dr. Black, *Address at York*, 1872.

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not statements of fact. And, in the words of Dr. Dudgeon, "the rule for the administration of the appropriate dose remains yet to be discovered."*

After the outline of the experiments recently made on small doses of drugs, given in the preceding chapter, you will, I think, already see that the impossibility of there being a law has vanished; that the improbability of its being discovered has also disappeared; and that instead of there being no connection between the dose and the principle, the connection is so close that it is one of absolute dependency—the principle itself being dependent upon the dose.

The law of *similia similibus curantur* belongs to, or is dependent upon, the action of a certain range of large doses; while the law of *contraria contrariis curantur* belongs to, or is dependent upon, the action of a certain range of small doses.

In other words, the curative action of small doses in disease and their physiological action in health are identical. The tendency of the drug in the small dose is towards the same action, whether the organ it acts upon is in a state of disease or of health.

The rule to which it is my happiness to invite your attention to-day has already been given you in the facts contained in the last chapter. It may be thus expressed:—

When a drug is prescribed as a remedy for a diseased organ, upon which it acts when taken in health, and for the kind of diseased action which, in certain large doses, it can produce in health, the dose must be small enough to be within the range of an action in the opposite direction.

For example—doses of *aconite* above a certain quantity cause a feverish pulse; doses below this certain quantity retard the pulse. The larger doses are injurious, the smaller are curative. Only the latter should be prescribed as remedies.

And so it is with every drug. It is simply a matter of experiment in health to find out for each drug the dividing line above which its action is hurtful, and below which it is curative.

This, of course, will be a work of labour and care; but so is the proving of drugs at all; and so is the practice of

* *Lectures*, page 560.

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medicine in any form. The advantage we have now gained is a clear perception of the work to be done, and how to do it.

You have now, I believe, the law of the dose; that in every case the dose chosen should be within the range of the small dose action, which, be it remembered, is always, as a matter of fact, contrary to the large dose action; and claims to be the only legitimate heir to whom the maxim *contraria* of right belongs.

In the early part of this address two similes were used which may now be perfected. One was that of a chain. It was said that Hahnemann's homœopathy—the symptoms of diseases, and the similar symptoms of drugs—form two links at the opposite ends of a chain. These two links are, therefore, separated by intervening links, which have hitherto been concealed. The connection which plainly exists between the two end links was unexplained, and the law of *similia* was an empirical law. Two of these intervening links were then described, first, the seat of the disease, and the seat of the action of the drug are the same; and second, the kind of action of both are the same. It was promised that a third link should be described by and bye. This third link has now been exhibited to you; it is the opposition of the action of the small dose to that of the large dose. These three links make plain to reason the connection between the links at each end. We have now a completed series of causation, and the law which was empirical becomes rational.

The other simile was that of a triangular basis and a three-sided column erected upon it, with inscriptions on each side. This must now be enlarged into a hexagonal foundation and a six-sided pillar. The six sides have six principles engraved on them, to forget for the moment the historical order of their discovery, in the following succession:—On the first side "in poison there is physic." On the second side "identity of seat of action of disease and o drug." On the third "identity of kind of action of disease and of large doses of the drug." On the fourth "similarity of the symptoms of disease and those of large doses of the remedy." On the fifth "contrariety of the action of small doses and large ones." And on the sixth "the dose of the remedy must always be within the limits of this contrary action."

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It may be asked, is there anything to guide our choice of a dose from among all those included within the range of the small dose action? This choice may be determined by reference:—

1. To the drug, and its primary and subordinate actions, *i.e.* to the organs in which these actions take place.

2. To the disease, and its special nature and susceptibilities.

For a drug to be a medicine it must have two actions in different doses: the action of the small dose must be contrary to the action of the large dose.

This suggests the idea that for the virulent poisons—such as *snake venom, arsenic, opium,* &c., for which no antidotes are yet known, the best antidote *may be* very small doses of itself. The only opportunity I have yet had of putting this thought (an hypothesis in the useful sense) to a practical test is in respect to mercury. In a case of poisoning by this metal the third trituration of itself (the millionth part of a grain) was manifestly beneficial.

The region of drugs as poisons in health, and as remedies in disease has been taken possession of. It is in part characterised by this—the hurtful action of drugs in health is perturbative; the curative action in disease is silent and peaceful. This is the geographical outline. The details are much "*terra incognita*" within, which need to be explored.

I am not prepared to enter into the investigation of the action of what are now familiarly known as infinitesimal doses. This part of the region is still mysterious. To a certain class of minds this mystery is its chief attraction. The haziness of its atmosphere renders it to them the valley of the enchantress, which has charms for such minds irresistibly attractive.

But I will offer one observation.—The action of really infinitesimal doses appears to be the converse of the action of the doses we have been occupied with to-day—one which is inappreciable in health, but perturbative, and sometimes curative in disease.

My brethren, I ought to have concluded long ago.

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Suffer me to offer you my respectful thanks for your prolonged attention. If I have succeeded in carrying your minds along with mine, or if you shall hereafter be brought to view these objects in the light which has been attempted to be thrown upon them to-day, the time and attention will not have been unprofitably spent, and the day and the place will have become memorable to you. Farewell!

Horton House, Rugby,
June, 1873.

Since this Essay was written I have received the 42nd 'Report of the British Association for the Advancement of Science' for the meeting at Brighton last year (1872), which contains a 'Report on the antagonism between the action of active substances by Thos. R. Fraser, M.D., Secretary to the Committee consisting of Sir R. Christison, Bart., Dr. Laycock, and Dr. Fraser.'

In this Report the results of experiments on the antidotal power of *atropia* (the active principle of belladonna) over the poisonous action of *physostigma* (Calabar bean) in rabbits and dogs are given. These results offer a confirmation to a certain extent of the conclusions drawn in this address as to the varied action of different ranges of doses of drugs. These experimenters found the limits within which sulphate of atropia can prevent the fatal effects of a lethal dose of physostigma. 0.005 gr. was too small, but "any dose ranging from 0.015 gr. to 5.2 grs" was sufficient. When the dose of physostigma was larger than the lethal dose, *e.g.* when *twice* the minimum lethal dose, the range was from 0.021 to 3.2 grs. When *two and a half* times the minimum, from 0.025 to 2.2 grs. When *thrice* the minimum, from 0.06 to 1.2 grs. And when *three and a half* times, from 0.1 to 0.2 gr. "Successful antagonism could not be obtained above this dose."

The conclusion of the Report contains an important statement:—

"An eminent authority in pharmacology has recently published the statement that the only method whereby the injurious action of a poison, absorbed into the blood can be made to terminate is by the employment of such means as will cause or hasten the elimination of the poison. This statement, fortunately, does not accurately describe our remedial resources. The existence of so undoubted

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an example of physiological antagonism as that between atropia and physostigma shows that the toxic influence of a morbific agent *may be directly opposed by a physiological antidote*, and that recovery may be produced by influencing the abnormal conditions themselves, in such a manner as to cause their return to a normal state."

July, 1873.

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ON SOME FORMS OF PHTHISIS PULMONALIS AND THEIR SPECIAL TREATMENT.

By H. Nankivell, M.D. Edin.

I shall endeavour to lay before you, as succinctly as may be, the chief points of practical importance in relation to phthisis pulmonalis, which have been brought to the front—I cannot say settled—by the recent investigations of German and English pathologists. And I shall also endeavour to indicate the importance of these views in their bearing on the prognosis and treatment, whether specific or hygienic, of that scourge of our race, consumption.

As you will see by the outline of the paper, which you already possess, I propose to discuss, first, the etiology and relationship of non-tubercular and tubercular phthisis. And I must guard these terms from being misunderstood, by saying that I intend by the first term that form of phthisis which is primarily not tubercular, but arises from an inflammatory process set up in the alveoli by the presence of some other exciting cause than tubercle; and by the second term I indicate that form of phthisis which is to all appearance of primary tubercular origin, or which is developed as a secondary complication in the course of a previously pneumonic phthisis.

Professor Niemeyer may be looked upon as the apostle of the following views, but his work has been based on and seconded by the labours of Cohnheim, Rindfleisch, Waldenburg, and others. Of late an able paper by Dr. Wilson Fox has thrown doubt on the soundness of these pathological theories; but seeing that they bear the test of clinical practice, which is the best of all tests, we cannot do better than accept them, and leave to histologists the attempt, in which I doubt not they will succeed, of reconciling microscopical appearances with clinical facts.

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A. *Etiology of Non-tubercular Phthisis.*

In a very large proportion of cases which we recognise under the term "pulmonary phthisis," the starting-point of the diseased condition is catarrhal pneumonia. Granted a certain amount of chronicity to this morbid process, and we pass by imperceptible degrees from what may be termed incipient phthisis to the establishment of a true first stage.

Catarrhal pneumonia may be set up in several ways, of which the following are the most frequent:—

(1) In the course of a bronchitis affecting the smaller bronchi, and characterised by the secretion of tough mucus, a small bronchus becomes plugged, and atelectasis of the lobule which is dependent on it for its supply of air occurs. The current of blood is thereupon obstructed, œdema sets in, and splenization is established: a condition of things most favourable for the supervention of catarrhal inflammation.

(2) Or else the bronchial catarrh insidiously extends itself to the alveoli, and an alveolar catarrh is set up throughout a certain number of lobules. An effusion occurs of a viscid character, rich in cells, which proliferate actively, and distend the alveoli: expectoration is also rendered more difficult, or even impossible, from the simultaneous obstruction of the bronchi.

These two causes are the most general and important, but catarrhal pneumonia may be set up also in the following ways:—

(3) After an attack of hœmoptysis: the whole of the effused blood is not expectorated, a portion having been drawn into the alveoli during acts of inspiration. It there acts as a foreign body: the air-cells become inflamed, and a catarrh is set up.

(4) Hyperæmia of the lungs, brought about by sudden, violent, or long-sustained exertion, has in many instances of persons engaged in usually sedentary occupation proved the starting-point of catarrhal pneumonia, either directly, or indirectly through hæmoptysis.

(5) The irritating presence of such foreign bodies as coal, iron, or stone dust must also be included in this list.

We have thus rapidly traced up the conditions named catarrhal pneumonia from their different causations, and have arrived at this point: one or more lobules, imper-

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vious to the air, blocked with an ever-multiplying secretion, for which there is but little chance of escape; the lung tissue proper becoming in turn compressed and devitalized. This point we must consider as the borderland between incipient and established phthisis; it is the limit of the absolute curability of phthisis. The secretion may alter and be partly absorbed or partly expectorated, and the lung tissue may in course of time resume its normal condition; or else a further stage of the disease is reached, termed CASEATION which consists of a peculiar fatty degeneration of the diseased tissues, and of the effusion which they contain; by this change the individuality of all the tissues involved is lost, with the exception of the elastic tissue and the large arteries. This process is then one of destruction, but preceding it, simultaneously with it, or following it, a process of conservation is in many cases established by a proliferation of the peri-bronchial and peri-vascular fibrous sheaths. The relative proportions maintained between these two processes continually varies: when caseation is in excess, we find "softening" occurring and the second stage of phthisis developing; when fibrosis is in excess, we get a chronic first stage and a tendency to cure by INDURATION; the caseous masses become encapsulated, and gradually shrink up with slight if any ulceration.

SOFTENING consists in the addition of fluid to the caseous solids, and then in a communication being established between them and the external air, by means of which the expectoration of the degenerated and semifluid tissues is brought about. This process may continue until the cheesy material has been excavated; or it may be arrested, and remain chronic for many years.

CASE I.—Four years ago a chronic case of softening was seen by me at Bournemouth: both apices were undergoing this process, and had been in a similar condition for six or eight years: occasionally small abscesses formed and burst, but no permanent cavity was established. This patient, when last I heard of him, was enjoying fair health at the Cape.

Softening is usually followed, however, by the establishment of the third stage, viz., an open cavity. Here we may hope for a third point of departure towards cure, viz., by *cicatrization*. A pyogenic membrane is formed to line the cavity, the surrounding tissues are protected

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from the more irritating effect of exposure to air, and contraction of the cavity commences, and may under careful management continue. The opposite alternative is the continuous enlargement of the cavity by inflammation and softening of the neighbouring tissue; or else by the deposit of true tubercle in these tissues, or generally throughout the lung.

We must not overlook the fact that croupous pneumonia may eventuate in abscess of the lung, and that from the time that a cavity is thus formed, the case practically becomes a third stage of phthisis: with this important distinction, that there is generally but one single lesion in the lung, and that a cavity succeeding croupous pneumonia has a great tendency to heal.

B. *Tubercular Phthisis.*

We will now pass to the discussion of true tubercular phthisis, *i.e.*, destruction of lung-tissue accompanied with the deposition of what is commonly known as miliary tubercle.

This deposit is found either as an apparently primary occurrence in the disease termed acute miliary tuberculosis, and here it exists in the disseminated form; or as a secondary complication arising in sequence to some caseous degeneration; and here it may assume, according to its intensity, either a disseminated or a localised form.

Some late observers have gone so far as to say that it is *never* of primary occurrence: that when it exists it does so in direct sequence to some previous point of caseation, either in lung, gland, or pleura; that where this point of caseous degeneration is apparently wanting, it has undergone such an amount of absorption or withering in the course of the disease as to be to all intents unrecognisable. These possibly extreme views receive support from the fact that the inoculation of animals with caseous matter induces miliary tuberculosis; and that too even in the case of Rodentia, a class which is particularly free from that disease.

If we carefully weigh these facts, and consider how seldom tuberculosis is to be found without the pre-deposition of caseous matter, and how frequently the inoculation of caseous matter has induced a crop of tubercle, we must acknowledge that the relation existing between these two conditions is generally one of cause and effect. We may

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for the present pass over, as not of extreme importance, the exact *modus operandi* of this relationship. Possibly the blood becomes tainted by the absorption of some poisonous material; or possibly even minute portions of the caseous matter may be introduced into the circulation, and tuberculosis be established in a manner analogous to pyæmia.

The deposition or growth of miliary tubercle appears to take place in the connective tissue of the lung, in the pleura, and submucous tissues; more especially around the capillaries, and also in the course of the lymphatics: its connection indeed with the lymphatic system will probably be shown by future investigation to be of the most intimate character, and to depend on changes of an inflammatory nature occurring in these tissues and in their surroundings. The effect of this deposit is to thicken and distort the alveolar septa, and to obliterate the air-cells by external pressure, which would of itself be sufficient to cause, after a time, the death of the part; but this process seldom continues long without catarrhal inflammation of the cells being set up, as *post-mortem* appearances generally show.

Disseminated miliary tubercle is typically found in the disease named acute tuberculosis: a disease of youth, marked by a persistently high temperature, and by a deposit of tubercle in the principle organs of the body.

Case II.—One such instance came under my care this summer in a girl of seven years, who had been under allopathic care for about six weeks. The urine was of an olive green colour, loaded with albumen and casts. There was a short cough; frequent sickness; great brain irritation; pulse 120 and temperature 103°; no expectoration, lungs resonant and respiration jerky. Under *bell.* and *ipec.* the vomiting ceased, the pulse and temperature both fell, the former to 90, the latter to 100°. Three days later hemiplegia and coma set in, and the child died in 36 hours. The autopsy revealed abundant miliary tubercle studding the brain, lungs, liver, kidneys, and peritoneum.

Of the localised tuberculosis of the lung we may consider that it runs an acute, chronic, or interrupted course; it is most generally, but probably not always, secondary to previous pneumonic or other caseous lesions.

As an instance of the acute form I give the following case:

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Case III.—C. D., æt. 32, unmarried, came under my care Sept. '72. When about 20 had an attack of fever and cough, with some trouble at the right apex. Chronic ill health had been more or less her lot; and she had always been treated allopathically. During the previous nine months had been getting worse, and therefore sought a change of treatment. She was thin and weak; pulse 90; temp. normal; follicular ulceration in the larynx; hacking cough and some hoarseness; chest sounds, except for feeble breathing in the right apex, normal.

During the next three months her health improved; pulse fell below 80, and the laryngeal ulcers disappeared. At this time she expectorated a calculus the size of a pea, evidently from the site of the former disease. In January, 1873, a febrile attack came on, the temp. rising to 104° and remaining there. Dulness and crepitation at the right apex became daily more marked; symptoms of a typhoid condition ensued, black tongue, petechial eruption, intense cerebral irritation; death occurred on the eleventh day, a cavity of considerable size having declared itself five days previously.

Case IV.—A case tending towards chronicity is reported by me in the *Brit. Journ. Hom.* Vol. XXX. p. 530. Shortly I may mention that the progression of deposit was at the margin of the healthy tissue; the exacerbations of fever and the new deposit took place during or after the menstrual period; with quiescence of the disease, and some restoration of strength in the intervals. The duration of this case from the first deposit in the right lung, which was probably pneumonic, was rather under three years; the progressive tubercular disease of the left lung existing during the last twelve months, and being complicated with intestinal ulceration during the last three months of life.

I may give the next case—I believe with tolerable assurance—as an instance of the *interrupted* deposit of tubercle.

Case V.—P. B., æt. 18, had just lost a sister of phthisis after four months' illness; had had a cough herself for two months, for which she had been sent to the south. She was seen in Feb. 1870. Had dulness under right clavicle of a decided character. For six weeks this steadily increased in extent, and then at several points showed a tendency to soften; expectoration occurring, and coarse

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clicks being here and there heard. At this time, however, the general health began to improve, flesh was put on; and in two months the signs of softening disappeared —the disease becoming quiescent.

This lady has wintered on the South coast, but not at Bournemouth, since that year. Though still an invalid, yet the disease has been for that time interrupted in its progress.

c. *Differential Diagnosis.*

The diagnosis between non-tubercular and tubercular phthisis is very important; and can in most cases be made with a tolerable degree of certainty.

In the first place the family history will show a tendency or otherwise to the development of phthisis; and if this tendency is strong, it will favour the probability of the lesion being tubercular, or being likely soon to become so.

The pre-occurrence of enlarged and suppurating glands will also favour the probability of the phthisis being tubercular.

A careful enquiry into the origin of the attack will then be made: if we cannot find a distinct trace of the occurrence of any of the causes of non-tubercular phthisis, we again conclude as to the probability of its being the more serious lesion.

The tone and ring of the voice will then be noticed: this will be often unaltered in the non-tubercular cases.

Finally, a careful comparison should be made between the general symptoms on the one hand, and the physical lesions on the other.

If a patient consults us, complaining of great loss of strength, evident shortness of breath on very slight exertion, a pulse over 100, and a temperature rising to 102°, and yet on careful examination we find very slight dulness of one or both apices, with slight clicking or crumpling to be heard in them with forced respiration, we ought to conclude that the disease is probably tubercular.

If, on the other hand, a fair ratio is maintained between these two classes of symptoms, and physical examination detects considerable dulness, coarse crepitation in abundance, or possibly a cavity of some size, we should trust that we had a non-tubercular case to deal with.

Two typical cases will illustrate my meaning well:

Case VI.—A., a lady, æt. 26, had for more than six

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months been nursing her father through a most severe illness, and had undergone loss of rest, fatigue, exposure to cold, &c. Had a bad cough for three months, and was getting thin and weak. Consulted me in 1871.

Her face was pinched and sallow; pulse 120 and very weak; respiration much quickened on exertion. Examination revealed extensive dulness beneath both clavicles, with coarse crepitation, in one place not much short of gurgling, and bronchophony. I gave a serious prognosis, and in consequence saw no more of my patient till I met her three months afterwards walking firmly uphill, and looking much fatter and healthier.

Case VII.—B., a girl, æt 18, consulted me in the summer of 1871. Had just lost a brother of rapid phthisis. Weak, muddy complexion, pearly eyes, scrapy hoarse voice. Pulse 120, small, weak; scraping, hacking cough; dyspnœa on exertion.

Examination showed very slight dulness in supra-scapular regions, with crumpling sound on coughing; no decided crepitation or vocal resonance; jerky respiration in several portions of the lung.

She had been previously under allopathic treatment. I watched her case for a month, and found it to be slowly retrograding—crepitation becoming established. She then returned to her own home and passed again under the care of her local medical attendant, and died in about three months.

D. *Prognosis.*

After what I have said I need not linger on the question of *Prognosis:* it is intimately bound up with the diagnosis.

In the non-tubercular class we may get recovery at all three stages—recovery by resolution, induration, or cicatrization; and this recovery may be permanent.

In the tubercular class the chance of recovery is much less; and the recovery is not so complete, and by no means so lasting: it is rather an arrestment than a true cessation of disease.

E. *Therapeutical Treatment.*

Turning now to the second division of my subject, viz., the therapeutics of non-tubercular cases, I claim for *Arsenic,* in the form especially of the *Iodide,* the first place in our list of remedies.

I believe it to be worthy of a trial in every such case

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when the exhibition of another remedy is not decidedly demanded; but I consider it to be indicated more especially in those cases of pneumonic phthisis where the bronchial symptoms have been decidedly prominent; and in those cases of *phthisis ab hæmoptoe* where the hæmoptysis has laid the foundation of the disease, and has no strong tendency to recur.

Its action appears to be beneficial both in incipient and advanced phthisis: the following cases will illustrate this.

Case VIII.—*a. Cure by resolution.* For full history, see *Brit. Journ. Hom.* Vol. XXX., p. 521. Consolidation of right apex, with commencing crepitation in front and behind; attacks of asthma had now and then occurred; improved under a four months' course, so that only very slight dulness and prolonged respiration remained in the suprascapular region. This patient has remained well since.

Case IX.—*b. Cure by induration.* For full history, see *Brit. Journ. Hom.* Vol. XXX., p. 521. Consolidation and coarse crepitation under left clavicle, with general bronchial irritation. This girl was under treatment for two months in 1870, and remains quite well still: slight flattening and dulness only continue.

Case X.—*c. Cure by cicatrization.* For full history, see *Brit. Journ. Hom.* Vol. XXX., p. 523. Cavity existed behind second and third ribs on the left side, but did not extend through to back. There was general bronchial irritation. The iodides of *arsenic* and *lime* were given alternate fortnights for the next three months: the cavity completely healed, and the cough disappeared.

Case XI.—The next is a case of hæmorrhagic phthisis, and I will give the history more at length. A lady, aged 39, after the birth of her 8th child, being out of health and having a cough, left her home in Gloucestershire for the sea-side. Within ten days, frightful hæmoptysis occurred. She was treated allopathically and in about three months was able to be moved to Bournemouth. This was in Oct. 1872. She was then thin and weak; very pale, nervous, short breathed on the slightest exertion: pulse 84; temp. 100°. Phys. exam. shewed flattening and dulness of the left chest, with spots of crepitation in front and behind; the dulness intensified at the axillary border of the great pectoral in front, and in the

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supra-scapular region behind. There was also some dulness beneath the right clavicle, where also prolonged expiration with increased vocal resonance obtained.

The *iodide* of *arsenic* was given throughout the whole winter in 6x, 3x and 2x preparations; the temperature slowly but surely fell to the normal point; the vocal resonance and dulness disappeared from the right apex; the left lung and chest decidedly contracted, the crepitations disappearing, and in some parts a respiratory murmur being established. Eighteen or twenty pounds was added to the weight. There was only once a slight recurrence of hæmorrhage.

Of the eleven cases which I related in my paper on "*Arsenic in Phthisis*" before referred to, I have no account to give of two (Nos. 8 and 9), having never heard since of their condition of health: one case (No. 11) remains in a precarious state; the other eight cases have enjoyed fair health and have kept free from any fresh attack since. These cases consisted of one incipient, two decided first stage, four second stage and one third stage; and the average duration of health which they have since already enjoyed amounts to 25½ months each.

Of the preparations of *lime*, I have seldom or never used the *carbonate* with advantage in phthisis. Yet its pathogenesy, its decided power over disorders of nutrition and glandular swellings would indicate that it has a large sphere of use in the disease under discussion. The *phosphate* I have used only occasionally, and I remember decided benefit to have followed its use in two cases only; in one of them—

Case XII.—A young man, with incipient phthisis, marked with deep sighing inspiration every two or three minutes, good was obtained.

Case XIII. In the second, a lady, aged 39, with chronic softening in the left lung, marked with slight purulent expectoration, and pretty frequent hæmoptysis of a scanty character, good also was obtained after the failure of *arsenic*.

The *iodide of lime* I have found beneficial in cases of the second stage where the moist râles are abundant, and in cavities where the secretion is excessive. I have often alternated it with *lycop.* with apparent good results.

Case XIV.—In 1868-69, soon after settling in Bournemouth, I was treating a lady the subject of advanced pro-

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gressing phthisis. A cavity of large size occupied the upper half of the left lung, the lower half being impervious to the air. On several occasions the iodide of lime dried the cavity in a remarkable manner and arrested its increase.

Another instance of its beneficial action I have already given when exemplifying interrupted tuberculosis of the lung. [Case V.]

Case XV.—A third case in which I found it most useful in conjunction with *lycop.* was in that of W. L., a waiter at the Railway Hotel in York. The case is published in full in the *Homœopathic World* for June, 1869. In six weeks a softening of the right apex was in this man much decreased both in extent and intensity: and the patient has been able for the past four years to carry on his arduous and unfavourable occupation at York without intermission.

The *arsenite of lime* I had used in several cases of the tabes mesenterica of infants with good effect, before using it in phthisis. It shares some of the therapeutical power of both its constituents, and as one might expect is useful especially in the third stage.

Case XVI.—A retired colonel came under my care in May, 1872: he had had a cavity for eleven years in the right apex. For the past two years an oozing of blood had occurred every two or three weeks from the walls of the cavity, giving rise to slight hæmoptysis, which was followed by some scanty purulent expectoration. The *calc. iod.* did partial good only; the *arsenite* acted well; after a six weeks' course of it, the hæmorrhages ceased to recur, and general strength greatly improved: he is now enjoying good health.

Case XVII.—A clergyman, aged 45, who had had pneumonic phthisis for years, with one or two severe hæmorrhages, consulted me in July, 1872, for a cavity in the left lung, which had been at least two years in formation.

Physical examination disclosed amphoric breathing and pectoriloquy in front of left chest from clavicle to upper edge of third rib, and posteriorly from apex to lower angle of scapula. The heart's apex was drawn about half an inch to the left of the left nipple, and the chest expanded less on the side of the lesion. The *arsenite of lime* (occasionally the *iod.* of *ars.*), was given all the winter; the patient had two slight attacks of hæmoptysis, probably

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caused by oozing from the walls of the cavity: improvement in the physical signs was steady; and in my last examination made at the end of August, 1873, I found no amphoric breathing or bronchophony in front at all, very slight and undecided behind, and that only in a very small space in centre of scapula. The apex of the heart is displaced very nearly across the axilla to within one inch of the lower angle of the scapula: and the anterior border of the right lung has extended $1\frac{1}{4}$ inch across the mesian line.

In all these cases, benefited by the preparations of lime, an absence of emaciation has been a marked feature; many of the patients have been plump considering the disease they were suffering from. They were generally pale, and there was tendency either to scrofulous swellings, mucous defluxion, or a flabby relaxed condition of the tissues.

Iodine I have seldom given alone in phthisis, but generally in alternation with its compounds of *arsenic* or *lime*.

The chief general symptom that calls for *iodine* is wasting,—the wasting that depends not so much on the pyrexia or the profuse expectoration, but on the inability to digest food, especially of an oily character. I have seen frequently a course of the 3x prep. enable a patient to digest milk with comfort, who had before been unable to take it. And the addition of five or ten drops of the 1x prep. to a pint of cod-liver oil solves the difficulty of taking it more readily and more effectually than either Fox or Agnew have done. When the dejections are undigested or fermented and semiliquid, this drug is also very valuable; even when the true diarrhœa of phthisis is setting in its action is useful. The presence of laryngeal and tracheal symptoms call also for its exhibition, and its local use is often valuable therein.

Lycopodium is a medicine I have not often given singly. Dr. Meyhoffer of Nice spoke lately at the B. H. S. of its value in chronic pneumonias, evidently referring to those of a catarrhal character. I am inclined to look upon it as an uncertain and temporary remedy, decidedly inferior to arsenic in curative power.

The physical indication for its use seems to be where one gets pretty extensive moist râles of a medium character with only comparative dulness: *i.e.* when the effusion into the air-cells has not yet become extreme, and where the

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affected lobules are probably not co-terminous with each other; the tendency to caseation and softening will here be but slight.

Sulphur I have scarcely ever used, except as an occasional intercurrent. When there is a suspicion of true tubercle, I question whether it is admissible in the lower preparations.

Of the class of remedies which occupy a more ephemeral position in the treatment of phthisis, we will first take *aconite*. The fever accompanying an intercurrent attack of catarrhal pneumonia does not always yield readily to this drug; and it certainly should not be pressed beyond twenty-four or thirty-six hours. It is useless to give it in the daily remittent that accompanies progressive phthisis.

Bryonia is a more valuable remedy, less depressing, and more in relation with the morbid processes. It often markedly relieves pain, dyspnœa, and cough; more recent deposits are much under its influence; pleural mischief especially calls for it.

Antim. tart. is valuable when there is profuse purulent secretion, and when coarse moist râles are abundant in the healthier portions of the lung. In the following circumstance it is invaluable: a patient with damaged lung, whom you may have seen in the morning, sends again at night in great distress, and dyspnœa. On examination yon will find, it may be, the lower lobe of the weak lung, though still resonant, to be devoid of respiratory murmur; the chest walls move with difficulty, and the intercostal spaces are drawn in during each effort at inspiration. This condition is caused by the blocking of a large bronchus, and is set right in a few hours by *ant. tart.*

I do not think *phosphorus* of much value in dispersing chronic deposits, or in aiding their induration. It assists, however, in checking the spread of the catarrhal process to fresh lobules, and also in stopping an intercurrent croupous pneumonia, and the pneumonia which succeeds to hæmoptysis. In later stages, where pulmonary exhaustion threatens, it is of great use, and relieves the accompanying dyspnœa.

The treatment of pulmonary hæmorrhage is not yet placed on a satisfactory basis: when the origin is bronchial and unaccompanied with fever, *hamamelis* will be found of most service and often of immediate value; where it is

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truly pulmonary and accompanied with fever, *acon.* and *hamam.* in rapid alternation are most useful. But when the hæmorrhage occurs during the process of softening, and most probably from a ruptured artery, *gallic acid* in full doses should be given, or a careful trial of Dr. Anstie's new remedy, *secale*, made. The *acetate of iron* is also very valuable in laryngeal and tracheal hæmorrhages with frequent tickling cough.

Another remedy which has a peculiar value in phthisis is *strychnine:* this alkaloid, in the form of the nitrate, in doses of the 200th to the 1000th of a grain, exercises its own peculiar action on the pneumogastric, preventing the frequent vomiting, and exciting the stomach to more regular and easy digestion; it has also the power in this dose of preventing much that is spasmodic in the cough, and of lessening the secretion from the lung and bronchi.

In doses of $\frac{1}{40}$ grain I have seen it act very marvellously on moribund cases, restoring for a few hours the power to think and act—a very valuable point on certain occasions.

F. *Hygiene and Diet.*

Coming now to the third part of my paper—the management of consumptive patients—I would say that it is especially the *patient* who has to be managed. Win his confidence, and enforce the obedience of your orders, and you may then hope to do something towards his cure. As a matter of fact he is generally far more docile at a health resort than when at home.

As to *Diet:* I order meat three times a day; a glass of warm milk, with or without rum, before rising in the morning; milk with an egg about 11 a.m., and milk again at supper time.

Of special foods three only require notice:—

(*a*) *Cod-liver oil* is by far the most valuable, and should be given in full doses; the "iodized" oil, as I mentioned, is often very easily taken and digested.

(*b*) *Pancreatic emulsion,* I have not frequently used: most patients find the oil less distasteful.

(*c*) The English extract of malt is a valuable adjunct, and supplies fat-forming material in a palatable and unstimulant manner.

Stimulants should be given, if at all, with great discrimination; malt liquors are preferable to wine or spirits. But if they flush the face or raise the pulse, they must be

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firmly withheld, whatever instinctive craving for them there may be.

As to *Exercise:* the one golden rule is, "Never get out of breath;" the second is, "Never get really fatigued." Unless these two rules are strictly kept, even to the letter, it were almost as well the patient never stepped out of a bath-chair. Whenever softening is going on, or there are recurrent hæmorrhagic attacks, even quiet walking on the level must be *carefully* indulged in. If we consider the physiology of the pulmonary circulation, the necessity of these rules becomes self-evident.

Lung Exercise should be taken twice a day, while sitting quietly, by drawing a succession of deep inspirations through the nostrils. The effect of this practice on debilitated lungs is remarkable.

As to *Rest:* Pyrexia is always an indication for *rest*, both of the general and pulmonary systems; but rest is also especially indicated when softening is in progress of an active character, or when recurrent hæmorrhages from whatever cause are present. Carefully decide that the amount of febrile action is dependent on certain active changes in the lung, and not on the general wasting,—and then order the necessary rest and see that it is carried out, even if it necessitates a week in bed.

As to *Air:* This should not be taken till two hours after sunrise, or later than one hour before sunset, either in summer or winter. Still, dry, cold air is decidedly preferable to warm moist air.

I should like to have spoken of clothing, baths, inunctions, mineral waters, and health resorts; but I have already transgressed the limit imposed on me, and will conclude this already too long paper by recording my increasing conviction that phthisis is daily passing more within the domain of therapeutics, and that if we take up the cases in their earlier stages and seriously treat them, a very large portion of success will attend our well-directed efforts.

Bournemouth.

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ON PYREXIA.

By J. Gibbs Blake, M.D. Lond.
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I.—*Definition.*

I purpose using this word in the sense that it is used by Dr. Sanderson in his admirable address on physiology, delivered at the last meeting of the British Medical Association, in the following paragraph:

"The first step is to select among the constituent phenomena of fever those which are the most important—in other words, to classify the characteristics of the febrile state according to the greater or less degree in which they are essential. This can of course be done on one principle only, that according to which those phenomena are regarded as most important which are more constantly present. On this principle we have no difficulty in placing pyrexia first; next comes, probably, loss of weight; next frequency of pulse, and after these other concomitant phenomena more or less dependent on them."

For several centuries pyrexia has been employed as a synonym of fever, and if the definition of fever were still that of Galen, "a preternatural heat," the two might still be used as synonymous.

Dr. Aitken (Vol. I. p. 29, 1866) defines fever as:

"A complex morbid state which accompanies many diseases as part of their phenomena, more or less constantly and regularly, but variously modified by the specific nature of the disease which it accompanies. It essentially consists in elevation of temperature, which must arise from an increased tissue-change, and have its immediate cause in alterations of the nervous system."

Obviously in these two quotations pyrexia is used in two senses; in the former it simply stands for a morbid elevation of temperature, whereas in the latter, although the elevation of temperature is an essential condition, yet

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all other symptoms of the complex morbid state are included in the definition.

The extensive use of the thermometer has brought about this limitation of the use of the word pyrexia. In a case of acute rheumatism, with a temperature of 109° Fahrenheit, although the skin is cool and covered with copious perspiration, we have to do with a condition more truly pyrexic than in a case of simple catarrh, with hot dry skin, and quick pulse, and temperature, it may be 102° Fahr.

In order to avoid confusion of terms, I intend, in this paper, to let pyrexia stand for "elevated temperature" which is the essential symptom of the febrile state.

II.—*Relation to Diseased States.*

In order properly to appreciate the effects of drugs upon the healthy and diseased body, as well as the relationship between pyrexia and disease, it is important to keep certain physiological facts in view, although it is not necessary to decide between the rival theories of physiologists as to the seat of the power of temperature regulation.

Naunyn and Quincke thought that they had demonstrated that the regulation centre exists in the intracranial portion of the spinal cord; Dr. H. Fischer concluded, from a number of experiments on the lower animals, that this centre is situated in the anterior columns of the cervical portion of the spinal cord, and Marey and Traube have each a vaso-moter theory of different mechanism.

We know not what important practical results may follow the settlement of the question involved in these theories, but independently of that decision, we can note the effect of certain substances upon the temperature of the body, and see how far these indications can be followed in practice.

The physiological facts referred to then are—

1. That animal body heat is derived exclusively from chemical changes. Human appliances being *storers*, not producers of heat.
2. In the febrile state the body is warmed faster than can be accounted for by accumulation. On this point the experiments of Liebermeister are conclusive.

Now, practically, we shall have to do with pyrexia as a

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symptom accompanying groups of symptoms which, for the sake of classification, are called diseases.

Elevation of temperature is observed in most acute and in many chronic diseases, and is a symptom of very great importance, but as the thermometer has been so recently applied to clinical medicine, it is not surprising that our Materia Medica is without indications of the elevations of temperature produced by drugs. The application of the thermometer, however, makes a change in the arrangement of future provings necessary. As we have seen that the essential symptom of fever is pyrexia, the rubric "fever" in our pathogenesis ought not to have such symptoms as "quick pulse," "dry skin," unless elevation of temperature co-exists.

It is not difficult to avoid the confusion of the heading fever. Let *temperature* be taken as a rubric, and under this head let all deviations from the normal temperature be placed together with the sensations of heat and cold and concomitants, but independent symptoms relating to perspiration should be transferred to "skin," which already contains many of them. "*Pulse*" should be a separate heading—properly placed next to "heart;" but as it is desirable to alter the order of the schema as little as possible, "pulse" might follow "temperature," and there would then be very little derangement of the present order. Facility of reference would be increased greatly by this change of arrangement.

The use of the thermometer is now so well established that it may seem a work of supererogation to prove its importance.

The very valuable aid that is afforded by means of this late addition of the application of physics to medicine is only second in importance to that obtained by the stethescope. At first it was urged as an argument against the use of the thermometer that it would take too much time; but I can confidently add my testimony to that of others, in this respect, that in the diagnosis of many cases great economy of time is effected. And what is still more important, especially in the case of children, is that an elevated temperature has many times set me to look for and discover some latent disease which otherwise would have escaped observation. In all cases of acute rheumatism, the attendant should be taught to use and read the thermometer, and give the warning of approaching danger.

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Before the thermometer was in general use, a case came under my observation of which the following are the main points. A gentleman between 50 and 60 years of age was attacked by acute rheumatism, of which he had had several attacks. The valves of the heart had suffered from former endocarditis; but although the heart was examined each day, no fresh endo- or pericarditis existed. The joints of the upper and lower extremities were affected, and the patient lay helpless in bed. *Acon.* 3 and *bry.* 3 were given alternately from the first visit, and on the third day he was seen at 3 p.m. Then he was going on well, apparently, but at 7 a.m. the following morning the medical attendant was hastily summoned, and learnt that the patient had begun to wander a little the evening before; but as there was not much pain, the nurse took little notice of this, the wandering went on to delirium, and the patient became comatose, with loud rattling respiration and cold sweat on the surface. In this state the patient was seen, and the temperature was then found to be 110° F. He died in a few minutes.

The danger to life in such cases is very great, and it is doubtful if any treatment would have saved this patient; still hydropathic treatment has been of great service in similar cases; and as we are not justified in using cold-water applications in acute rheumatism, unless the temperature becomes hyper-pyrexic, it is important to get early notice of the tendency to a high range of temperature. The thermometer really tells of the degree of severity of the affection, and in some cases it affords equally positive evidence in diagnosis. I will illustrate this by a case which I observed last year. At midday on 12th May, 1872, I was sent for to see a lad of 17, whom I found in bed, with a pulse of 120, and temp. 103° F. For three days he had been complaining a little of headache and general malaise, chilliness and feverishness, but no backache. I examined his abdomen and found a red papule of the size of a pin's head, fading on pressure, and the abdomen somewhat distended. I told the friends I suspected typhoid fever, although I was puzzled to account for the rash at so early a period. I decided to go again at night, and at 9 p.m. the temperature had fallen half a degree. I then withdrew my diagnosis of typhoid fever, but had to confess that I was puzzled. On the morning of the 13th May the pulse was 140 and the temp. 103° F.,

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and the diagnosis was plain; he was covered with shot-like papulæ—the boy had small-pox. It became confluent, and on the 15th May he was in violent delirium. He made a good recovery, taking *tart. emet.* 2x for the greater part of the time.

During the first four days of the initial stage of typhoid fever the temperature always rises higher in the evening than in the morning, and the falling temperature of the evening was therefore a strong argument against its being a case of typhoid fever.

The total absence of anything like pain in the back was a remarkable symptom in this case, and the appearance of the first papule on the abdomen, a little to the left and below the umbilicus, was also peculiar; and it is worthy of remark that this papule became pustular before those on the face, and thus retained the start that it had at the beginning.

In addition to the value of the thermometer in diagnosis and prognosis, I may urge its value during convalescence. After scarlet fever, sequelæ are announced by elevated temperature, and the tendency combated by avoidance of exposure.

In the treatment of disease, careful thermometric observations are of great value in enabling us to judge of the value of therapeutic agents.

III.—*The Effect of Medicinal Substances on the Temperature of the Healthy Body.*

Action and reaction are as dependent one upon the other as much in pharmacology as in physics, so that in this section it is necessary to take notice of depressions as well as of elevations of temperature.

It is as important for our purpose to note the kind and direction of action, as it is to observe the parts and functions of the body acted upon. It will, however, be found very difficult in some cases to decide whether a certain drug elevates or depresses temperature, because many substances elevate the temperature in one dose and depress it in another dose, and some again depress the temperature at first and afterwards elevate it, and *vice versâ*. Hence drugs cannot be divided into pyrogonics and non-pyrogonics, but each one must have its separate groups of symptoms, which form its pathogenesy.

I have collected together for this section a number of

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observations made by physiologists, upon the temperature observed in man and other animals under the influence of certain drugs.

Aconite and Aconitine.

Achscharumow found that a quantity of *aconitine* varying from $\frac{1}{100}$ to $\frac{3}{100}$ of a gramme, subcutaneously injected, produced salivation, dyspnœa, slight cyanosis, diminution of heart's beat and of the temperature, and paralysis of the extremities. In the cases that recovered, the animal remained as if asleep from two to four hours. When death takes place, clonic convulsions precede it.

The following conclusions relative to the action of *aconitine* have been arrived at by Dr. Achscharumow. (Reichert and Du Bois Raymond's *Archives*, No. 2, 1866.)

1st. Death is not due to paralysis of the respiratory organs, but to stoppage of the heart from paralysis of its motor ganglia.

2nd. The first effect is irritation of the medulla oblongata, which is communicated to the vagi.

3rd. Paralysis of the vagi supervenes from continued irritation.

4th. The temperature and blood pressure are lowered.

5th. The peripheral nerve endings and trunks are entirely paralysed.

6th. Reflex function of the spinal cord remains intact.

7th. The brain is unaffected.

8th. It has no local action on the pupil.

It is unfortunate for our purpose that physiologists in Germany have experimented principally with *aconitine*, which is defined by them to be purely a narcotic, and which exists in *a. napellus* in conjunction with the other alkaloids which have different actions on the human body. Dr. Ringer's description of the action of the whole plant is more to our purpose, but I cannot find any thermometrical observations as the result of experiments with the whole plant, *aconitum napellus*.

There is a great difference between the summary of the action of the whole drug, as given by Ringer, and that of the alkaloid, *aconitine*. But in none of the descriptions of the action of *aconite* do we find any great increase of temperature. At first, with moderate doses in healthy subjects, there is observed a feeling of warmth and slight acceleration of pulse, followed by diminishing frequency of pulse and respiration and temperature, which may last

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many hours or even days; then comes a reaction, and the circulation, respiration and general temperature are somewhat increased.

It will appear that many drugs with much less or even no reputation in the treatment of the febrile state, produce much more disturbance of the thermometric condition than does *aconite*—as for instance, *atropia*.

Nitrite of Amyl.

Dr. Horatio Wood thinks that by checking tissue change it lowers temperature of warm-blooded animals; and the following experiments, extracted from the *American Journal of Medical Science*, July 1871, show that the nitrite is simply a temperature depressor, so that we can have no hope of its use for pyrexia on the principle of similars. On the contrary, Hayden aud Cruise found that in the collapse of cholera the temperature rose from 95⅕ Fahr. to 96¾, after the inhalation of the nitrite for three minutes. (Quoted by Dr. Ogle, *Clin. Soc. Trans.*, Vol. V.)

Exp. 33.—A large female cat. 3.40. Thermometer introduced through abdominal walls indicated 103¾ F. 3.43. 12 gtt. of the nitrite were put on inhaler, and it placed over cat's face. 3.48. Temp. 101½°.

Exp. 34.—Jan. 6th. Adult pigeon. 4.25. Temp. 107½°. 4.26. Injected into breast 2½ minims of the nitrite. 4.36. Not as yet materially affected; temp. 106°. 5.0. Temp. 104°; no symptoms save quietness. 5.40. Temp. 102°; no symptoms manifested. 6.15. Temp. 103⅕°.

Jan. 7th. Seems perfectly conscious; no soreness of breast manifested.

Exp. 35.—Jan. 9th. Same pigeon as in last experiment. 12 p.m. Temp. when brought out of yard, 109⅓° 12.20. Injected about 5 minims. 12.28. Walks very staggeringly, with head down; temp. 108°. 12.40. Perfectly conscious, but unable fairly to walk. 12.49. Conscious, quiet, breathing very deeply; temp. 103°. 1.5 p.m. Temp. 101°. 1.25. Seems more lively, but unable to walk; temp. 97°. 2.10. Temp. 95°; just had a short, sudden, very violent convulsion, in which he died.

Exp. 39.—A young rabbit. 4.30. Temperature in rectum, 102°. 4.34. Injected 8 drops. 4.40. Temp. 100¾°. 4.45. Temp. 101½°; injected 12 drops. 4.48. Breathing hurried, panting; temp. 99°. 5. Temp. 97¾°. 5.4. Temp. 97½°. 5.9. Temp. 97°. 5.10. Dead.

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Belladonna and Atropine.

Belladonna, according to Meuriot (*Practitioner,* Vol. I. p. 195), causes all its action on the brain, not by an elective affinity for the brain tissue, but as a consequence of the disturbance of the circulation which is brought about. He has carefully noted the effects in temperature, and finds that in man the temp. is raised from ·5° to 1·1°. In animals he has found that small doses increase and large ones diminish temperature; thus he has obtained an increase of 2 to 3°, and a diminution of 4 or 5. The action of the heart keeps pace with the temperature.

The following experiments of Dr. Oglesby (*Pract.* Vol. IV. p. 31) shew that the action of atropia is more constant as an elevator of temperature than is *belladonna,* although in experiment 2 the temperature sank considerably after a slight rise.

Exp. 1.—A large rabbit, 12 hours after a meal, had a temp. of 103°. At 6 a.m. was injected hypodermically with a solution containing one sixth of a grain of *sulphate of atropia.*

A.M.	Temp.
7.25	102·1°
8.40	102·0°
9.50	103·1°
11.40	103·3°
12.40 P.M.	104·0°
2	104·1°
5.1	103·3°
8	103·0°

Exp. 2.—Twelve hours after a meal a healthy and well grown rabbit had a temp. of 103·3°. At 7 a.m. was injected beneath the skin with a solution of *sulphate of atropia* containing $\frac{1}{6}$ of a grain.

A.M.	Temp.
7.35	103·1°
8.50	104·2°
10	104·43°
11	104·0°
3 P.M.	102·2°
6	101·1°
10	99·4°

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At this time the rabbit was seized with violent convulsions which lasted until the time of its death, 30 hours after the administration of the poison. For several hours previous to death the temperature fell very steadily, and a few minutes before death the thermometer registered 94·2°.

A post mortem examination, made shortly after death, showed considerable discoloration of the muscles of the back in the lower dorsal and lumbar region from extravasation of blood. On examining the cord and its membranes, an effusion of blood between the membranes and cord was formed at a point corresponding to one in the muscles of the back. Around the medulla there was a thin layer of blood. The organs of the body were quite healthy, with the exception of the right lung which was slightly congested.

Exp. 3.—A guinea pig, after fasting 12 hours, had a temp. of 102·1°. At 7.20 a.m. a solution containing $\frac{1}{40}$ of a grain of *sulphate of atropia* was injected beneath the skin.

A.M.	Temp.
8	101·4°
8.30	101·0°
9	102·3°
9.30	102·4°
10	103·0°
11	103·2°
12	103·4°
1	103·1°
2	102·4°
3	102·4°

Exp. 4.—A strong rabbit, 10 hours after a meal, had a temp. of 103°. Injected beneath the skin at 6 a.m. with a solution containing $\frac{1}{50}$ of a grain of *sulph. of atropia.*

A.M.	Temp.
6.20	102·3°
6.50	102·1°
7.30	102·2°
8.30	103·1°
9.30	103·2°
2.30 P.M.	101·4°

Animal fed at 2.40 p.m.

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A.M	Temp.
4	102·0°
5	102·3°
6.30	103·3°

Exp. 5.—A rabbit, 4 hours after a meal, had a temp. 103°. At 2.30 p.m. injected beneath the skin with a solution containing $\frac{1}{40}$ of a grain of *sulph. of atropia.*

P.M.	Temp.
3	102·3°
3.20	102·3°
4	102·3°
5	103·0°
6	103·1°
7	103·1°
8	103·2°
9	103·1°
10	103·0°

Exp. 6.—A cat, immediately after a meal, had a temp. 103°. Injected at 6 p.m. with a solution containing $\frac{1}{60}$ of a *grain of atropia.*

A.M.	Temp.
6.15	102·4°
6.30	102·3°
7	102·3°
7.30	102·2°
8	102·4°
8.30	103·1°
9.30	103·2°
10.30	103·2°
11.30	104·0°
12.30 P.M.	104·0°
1.30	104·0°
3	103·4°
7	103·1°

Calcarea.

Lime salts injected into a vein elevate the temperature and quicken the heart's action, and if the quantity be large paralyze the heart; but *carbonate of ammonia* injected into the blood causes diminution of the temperature.

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Calabar Bean.

Dr. Fraser found that temperature was slightly raised; that the minute blood vessels of the frog's foot are first considerably contracted and then dilated, and considers that the latter at any rate is due to the specific effect of poison upon the ganglia and nerves that govern the calibre changes of the vascular system.

The pupil is slightly dilated before the great contraction takes place. We have indications in this medicine of considerable value, and few drugs have been more exhaustively proved by physiologists. It should have a place in our new Materia Medica.

Camphor.

Temperature raised (*Wunderlich*). A case quoted in *Jahresbericht*, 1870, Vol. I, p. 1354, a child 3 years old. Five grains, given as a clyster, caused cyanosis; violent retching; cold sweat; convulsions; anuria (? suppression); stupor and prostration for 10 hours.

Chloral

Produces considerable depression of temperature in health, but in cases of fever the effect is very variable. In some cases it had no effect, in others again the temp. was reduced from 103° F. to 99° F.

Coffee.

Action of caffeine (*Jahresbericht*, 1868, Vol. I, p. 357). Brown-Séquard finds that caffeine causes convulsive movements of the extremities; irritation and quickening of the heart and respiration movements; increased blood-pressure; irritation of brain and spinal cord. Muscular irritability is destroyed. Strong coffee causes an elevation of temp. which reaches its maxium $\frac{5}{4}$° Fahr. in an hour.

Gelseminum

Reduces the temperature very decidedly in the pigeon and the cat, and the generni toxic effects of *gels.* are the same upon man as they are upon other warm blooded animals.

Disorders of motility and sensibility.

Laboured respiration.

Weakened action of the heart.

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Dilated pupil; double vision; ptosis.
Intellect unaffected till near death.

Pract. Vol. V. p. 207.

Dr. Robert Bartholow's Experiments.

Exp. 5, *Pigeon.*—Temp. of gullet 107° Fahr. Respirations 48. Injected under integument of thigh thirty minims of the solution of *gelseminum.* At the end of five minutes voluntary movements of walking disordered; respiration 30 per minute laboured, expiration jerking and prolonged. In ten minutes legs were paralysed, so that standing was no longer possible. Soon after wings were widely expanded, resting on the floor and agitated by a succession of short tremors which at length extended from the wings to the whole body. Eyelids partly closed. Still manifested consciousness and attempted to move away head when approached. Sensibility was at length completely abolished, so that no form of irritant excited resistance or movement. Death occurred at the end of half an hour in a general convulsive tremor in which the eyes were closed, the head drawn down, the feet extended backward, and the wings widely expanded. The action of the heart continued for several seconds after the total suspension of the respiratory movements. Just before respiration ceased the thermometer introduced into the gullet indicated a temperature of 104° Fahr.

Exp. 6. *Kitten.*—Temp. of axilla 102° Fahr. Injected 30 minims of solution of *gelseminum.* In ten minutes head depressed, resting on forelegs which were doubled up; expiration jerking; the abdominal wall falling in toward diaphragm suddenly; pupils dilating, and eyelids drooping. In attempting to walk fore legs are weak and relaxed, but hind legs are less affected; when tail is pinched cat cries out and attempts, but ineffectually, to strike with fore legs; lips and tongue are dry, and tongue is frequently protruded; keeps tail in constant vibration. In 15 minutes after injection began a series of backward movements which were repeated irregularly every few minutes. This backward movement is accomplished chiefly by the hind extremities, the claws of the fore feet catching in the floor in consequence of the loss of power to retract them. The cat is yet conscious of impressions, for reflex wrinkling of the eyelid takes place on touching

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ear and face, and she cries out when the tail is strongly pinched. At the end of 20 minutes the convulsive backward movements are more frequent and irregular. Jaws are widely separated, mouth parched; fore legs are drawn up, and folded under the chest and the hind legs are rapidly flexed and extended without, at last, moving the body. In 30 minutes after the injection, there is complete muscular relaxation; respiration ceases, but the heart continues to beat for five minutes longer. Temp. of axilla before respiration ceased 98° Fahr.

Daturine and hyoscyamine

In small doses slightly increase the temperature, and in large doses slightly decrease the temperature.

Ipecacuanha and its Alkaloid Emetia.

Emetia caused in a rabbit, (normal temperature 103°) in 15 minutes 103°5 coolness of surface; in 6 hours 100°5.

The action of *emetia* is well illustrated by Dr. Duckworth's experiments. *St. Barth. Hosp. Rep.* Vol. 5, p. 222.

Exp. 8.—Injected gr. ½ into rectum of same rabbit. Some of the solution was lost during the operation. Distinct sedative effect at once noticed. Animal refused food and was purged. Remained quiet in one place, breathing hurriedly, 88 per minute. Great depression. Eyes half closed. Temp. of rectum 101° F. (blood on thermometer on withdrawal); in axilla 100° F.; death in 45 hours; sectio in 14 hrs. Some blood-stained fluid in both pleuræ, Lungs, emphysema in parts, some scattered patches of lobular pneumonia, granular on fracture; some lobes reddened and consolidated sinking in water. Trachea not much injected; bronchi contained some reddish fluid, no froth. Heart, all cavities contained dark clots. Blood throughout the body mostly coagulated. Liver congested in its portal system. Spleen and kidneys not remarkable. Stomach full of green food; mucous membrane softened and pale. Rectum reddened and inflamed, bowel above this for several inches congested, and with much free shiny mucus on its surface. Some clear yellow urine in bladder albuminous, acid. Tr. gallæ caused a precipitate.

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Free tesselated epithelium and granular matter on microscopical examination ; no blood discs.

Expt. 13.—Gave gr. j. mixed with bread to a large rabbit. Immediate sedative effect noticed, and these continued to the time of death, which occurred within 18 hrs. The animal ate some food during the day. Sectio in 30 hours. Body found lying on belly, rigor passing off. A little clear fluid in each pleura and in pericardium. Lungs deeply red and congested in several lobes, especially in anterior portion. Pallor for the most part at the roots. (This was easily explained by the position of the body for so long after death.) The darkest portions of the lungs sank in water. Trachea injected, and of vivid crimson colour. This congestion ceased somewhat abruptly just beyond the bifurcation of the bronchi. Small tubes filled with frothy serum, also more injected than was natural. Heart, all cavities distended with dark coagula; vena cava also. Some fluid blood in thoracic aorta, less dark. Stomach distended with green pultaceous matter, which was mostly covered by a layer of shed mucous membrane, probably the result of post-mortem softening. No congestion, nor was any met with in the duodenum or remainder of intestinal tract. Rectum empty and not contracted. Liver, spleen, and kidneys natural. About fl. 3 j. of acid turbid urine was found in the bladder, and proved to be highly albuminous. Tr. gallæ threw down a copious ppt. from it. Brain apparently natural.

Expt. 18.—Injected gr. j. under skin of a large cat. Vomiting and purging, followed by general sedative effect. In 24 hours breathing shallow, feeble, and hurried. Marked prostration. Gave gr. j. by mouth. Death within 30 hours, sectio in 13 hours. Rigor. Ecchymoses at points of injection under skin. Lungs congested, collapsed in parts, and a little emphysematous. Trachea slightly injected; abundant frothy serum in bronchi. Heart, right side moderately distended with dark blood; less so on left side. No effusions into serous sacs. Stomach, cardiac end, driven through the diaphragm, owing to violent vomiting, contracted and somewhat congested. Some of Peyer's patches injected. Cœcum vividly congested, empty. Large bowel only slightly congested; empty, but with excess of mucus. Bladder contracted, congested, and empty. Liver congested in centres of lobules ; gall bladder not empty.

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Opium and its Alkaloids.

I can find no record of observations on temperature in cases of opium poisoning, but it will be seen from the following experiments that the elevation of temperature caused by *morphia* shows that it is possible that the action of the *calomel* and *opium* may have formed in the latter a truly homœopathic reducer of febrile action.

A small dose of *morphia* hypodermically injected quickly lowers the temperature, but the depression is of short duration, and is followed by a steady rise, which lasts for hours. This was observed in the human subject, and the observation is confirmed by the following experiments. Incidentally it may be mentioned that small doses of *morphia* improve the appetite, and it is well known that large doses produce anorexia.

Expt. 1.—A healthy young cat, deprived of food for several hours, had a temperature of 101°.

At 9.30 a.m. a solution of the *bimeconate of morphia* containing one-eighth of a grain, was injected beneath the skin.

A.M.	Temp.
10	99·1°
11	99·0°
12	99·4°
1 P.M.	100·3°
2	102·1°
4	102·3°

In the foregoing case the effect of the dose was very marked, the cat being much affected by it, jumping and running round the room and uttering plaintive cries. The increase of temperature lasted many hours.

Expt. 2.—A guinea-pig, four hours after a meal, had a temp. of 101·2°.

At four P.M. it was injected beneath the skin with a solution of *acetate of morphia*, containing one-eighth grain.

P.M.	Temp.
4.30	101·2°
5.0	102·2°
5.30	102·4°
6.0	103·0°
8.0	104·1°

In this case 104·1° was the maximum degree attained.

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This degree was maintained, with but little alteration, until 10 P.M., when there was a very sudden and progressive fall to rather below the normal standard, probably owing to the long fast the animal had undergone.

Expt. 3.—A strong terrier, three hours after a meal, had a temp. of 102·3°. At 4.30 P.M. was injected hypodermically with a solution containing one-eighth of a grain of *morphia*.

P.M.	Temp.
5.0	101·4°
5.30	101·1°
6.0	101 1°
6.30	101·4°
7.0	102·1°
8.0	102·4°
9.0	103·1°

The maximum temperature in this case was steadily maintained for several hours, although the dog from time to time vomited freely.

Expt. 4.—A guinea-pig, after fasting ten hours, had a temp. of 102·1°. Injected at 6 P.M. with one-sixth grain of *morphia* beneath the skin.

P.M.	Temp.
6.30	102·1°
7.0	102·1°
7.30	103·2°
9.0	104·1°
11.0	103·0°
12.0 A.M.	102·3°

Expt. 5.—A rabbit, one hour after a hearty meal, had a temp. of 103°. A solution containing one-eighth of a grain of *acetate of morphia* was shortly afterwards (12.30 P.M.) injected hypodermically.

P.M.	Temp.
1.0	103·1°
2.30	102·3°
3.30	102·4°
5.0	103·3°
6.0	103·4°
7.0	104·0°
8.0	103·2°

Expt. 6.—A strong cat, after fasting for several hours had a temp. of 102°. At 3.30 P.M. it was injected beneath

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the skin with a solution containing one-fourth of a grain of *acetate of morphia.*

P.M.	Temp.
4.10	101·3°
4.35	104·0°
6.50	104·1°
8.0	104·2°
10.0	104·2°
11.0	103·1°
11.30	103·1°
12.0	102·3°

The large dose of a quarter of a grain had a very marked effect on the condition of the cat. It had many violent convulsions, and an inability to use its hind legs properly. This condition did not pass off till the following day.—Dr. Oglesby, *Pract.*, Vol. IV., p. 28.

Phosphorus

Causes variations of temperature—at first a slight rise, then a depression, then again an elevation, followed by considerable depression.—Virchow's *Archiv.*, 1870, p. 457.

The following is given in Dr. John Ogle's article on "The Temperature in Nervous Diseases" (*Trans. Clin. Soc.*, Vol. V., p. 83) :—

In a case of acute poisoning by *phosphorus* related by Dr. Habershon in the *Medico-Chirurgical Transactions*, Vol. II., p. 87, the patient, a woman, æt. 28, was brought into the hospital five days after the poison was taken, and the temperature was 89·8°, four hours after it was 91·5°. She died in $8\frac{1}{4}$ hrs. after admission. In Dr. Tüngel's *Klinische Mittheilungen von der Medicinischen Abtheilung der Allgemeinen Krankenhaus in Hamburg* for 1861, are related (see p. 125) sundry cases of poisoning by the drinking of liquids in which phosphorus of lucifer matches had been dissolved. In these, taken together, the pulse and temperature showed by no means any great or constant variations; but one or two of the cases are interesting in this respect. Thus, in Case 12, the temperature when the patient was admitted was 100·85° Fahr. (30·6° R.) Three days later it was 99·5° F. (29·8° R.) In the evening of that day it was normal. On the evening of the next day it was 97·25° F. (29° R.) Later on it was normal. She recovered. In Case 13, when admitted

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three days after drinking the poison, the temperature was 97·7° F. (29·2° R.) On the following morning it was only 28·2° R. On the next day it was 97·25° F. (29° R.), and in the evening 97·7° F. (29·2° R.) It afterwards rose to 99·95° F.; and after diminishing again 100·4° F. she recovered. In other cases the temperature was not apparently affected. In Case 18, the temperature numbers were as follows:—At first 100·85° F.; then 99·95° F.; then 100·4° F.; then only 95° F.; being in the evening 98·15°; then 97·25° F.

Verat. Viride.

Dr. Squarrey's Experiments on Convalescent Patients.—No effect of importance was noticed on the temperature, but the heart's beats were diminished in force and frequency, and the respiration lessened. Nausea and vomiting were produced when more than 20 minims were given at a dose, and great weakness.—*Pract.*, Vol. IV., p. 211.

Woorara.

In lower animals the injection of this substance produces decrease of temperature till cramps set in; the increase probably owing to the heat produced by muscular action. —*Tscheschichin.*

Voisin and Leonville, by means of subcutaneous injections of this drug, induced a complete artificial fever in human beings, with rigors, heats, and sweatings, the temperature rising to 104·8° F., accompanied with all the signs of febrile circulation and secretion and disturbance of the nervous system.—*Wunderlich*, pp. 139–140.

Fleischer (Plüger's *Archiv.*, 1869, p. 441) confirms Voisin and Leonville's experiments.

Goujon found in repeated experiments on rabbits with *curare* that the temperature was always raised, and at the same time symptoms of exophthalmos; and Bert found sugar in the urine and saliva of a dog poisoned with *curare.—Jahresbericht in der Gesammten Medicin*, Band I., p. 360, 1870.

The subcutaneous injection of this substance in very minute doses deserves a trial in cases of elevated temperature attended by a profound disturbance of the nervous system. In such cases the power of absorption of fluids from the stomach may be suspended, and *woorara* has been more experimented upon by subcutaneous injection than by the mouth.

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Moschus raises temperature. *Strychnine* and *bromine* also raise the temperature. *Picrotoxine* and *nicotine* cause a primary elevation, followed by a depression of temperature; and *quinine* seems to have the power of raising the temperature in health, whilst in large doses it depresses the temperature in disease; but I can do no more than enumerate these substances and indicate their direction of action, as already too little space is left for the section on treatment.

V.—*Treatment.*

Professor Humphrey, in the address delivered a month ago to the Physiological Section of the British Medical Association, makes the following startling statement:—

"The physiologically antagonistic influences of *atropia* and *physostigma*, and the fact that a poisonous dose of the one may be given with impunity if the other is administered along with it, seem to open a prospect that really curative—that is, antidotal—agents may be discovered, not simply for drugs, but for the effects induced by drugs, and also for the changes which constitute disease; and the observation that *morphia*, *chloroform*, and some other substances, produce different and sometimes opposite effects, according to the doses in which they are given, renders it not improbable that poisonous agents may, in some instances, be antidotal to themselves, and that the word "homœopathy" may be rescued from its position as the expression of a fallacy, and may yet take its place in the etymology of scientific medicine."

I say startling, because Professor Humphrey is prepared to state that all that is common to the homœopathic body is "*not improbable*," and because he has gone so far and not discovered the fact that the definition of homœopathy adopted by its practitioners is that implied in the first part of his sentence.

One of the first inferences that will be drawn from a comparison of the fragmentary provings of the last section will be that, in the treatment of the febrile state, we ought not to be confined to *aconite*. There ought to be no need for the alternation of this medicine with another medicine when that other contains pyrexia (elevated temperature) in its pathogenesy, and there is little doubt that further experiment will materially add to the number of drugs that have this power of acting on the nerves that

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regulate temperature. I have no wish to undervalue the power of *aconite* in the treatment of the febrile state, especially at the commencement of illness; but I wish to protest against its being considered the sole remedy for feverishness.

In my experience I have found *aconite* of no value in the form of acute rheumatism accompanied by hyperpyrexia, and it has failed in the hands of others who have used it in heroic doses.

The *medicine* for this state has still to be found; and since I have looked up the physiological actions, I have not had an opportunity of trying in hyperpyrexia any of the drugs that produce high temperature.

During the course of acute rheumatism, with moderately-elevated temperature, and inflammation of the spinal membranes, *atropia* 2x, was of marked benefit in one case.

In chronic elevation of temperature, increasing towards evening, and accompanied by tuberculosis of the lung, I have used *calc. carb.* 3x, and continued it for several weeks, apparently with decidedly good results. The evening temperature reached 103° Fahr. in one case. But I need hardly state that this fails in many cases to bring down the temperature.*

I have already far exceeded the limits allowed to readers of papers, so that I must postpone the consideration of several drugs, such as *bryonia* and *baptisia*. Some temperature charts, which I have prepared, give the main points of the cases; and I will conclude with a few remarks on the use of cold water and galvanism.

Water.

Up to the present time the only treatment that is of service in the management of a case of hyperpyrexia is hydropathic. If a physician is called to a case in which the temperature of the axilla is more than 108° Fahr., no medicine hitherto tried is of any service—the patient is comatose, breathing rapidly, with mucus rattling in the throat, and apparently dying; and death will inevitably result in a few hours, unless the temperature be reduced. If the patient is found at this step, the temperature must be reduced

* Some charts of cases treated in the Mason Orphanage show the action of *calcarea* in diarrhœa and debility with high temperature, kindly supplied by Dr. Wynne Thomas.

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quickly; and the best way to do this is to place the whole body into a bath of 90° F., and reduce the temperature of the bath with cold water and ice. In this way the bodily temperature may be reduced to 98°, and if the coma has not lasted long, the patient may recover consciousness in the bath. But if the temperature is 106° or 107° in the axilla, the "pack" with towel may be sufficient, but if that does not quickly reduce the temperature, the whole body must be packed in a wet sheet. By these means the excessive temperature can be reduced, whilst the general symptoms are treated by medicines.

Galvanism

May be required in the treatment of hyperpyrexia. During the comatose state the breathing may be arrested, whilst the heart goes on beating regularly; at first the reflex actions caused by splashing the face with cold water are sufficient to arouse the action of respiration, as in the new-born child; but after awhile this stimulus ceases to have any effect, and the application of the induced current to the trapezius and sterno-cleido-mastoid *on the right* side will cause the act of respiration to be resumed. The left side should not be galvanised, lest the action of the heart be affected injuriously.

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ON THE THERAPEUTIC PART OF THE BRITISH REPERTORY.

By P. Proctor, Esq.

The object of the remarks I have to make is to introduce to the Congress the very practical and important subject of the proposed therapeutic part of the Hahnemann Publishing Society's Repertory; and as many present may not be well acquainted with the scheme in its present condition, I propose glancing at some of the points, in the hope that they may then be discussed with advantage and a definite plan agreed upon, with a working organisation, without further loss of time.

The educational requirements of homœopathy are, in the first place, the statutes at large, viz., the full records of medicinal provings, without a single comment to bias the judgment; 2ndly, a repertory or classified list of the symptoms, for the sake of easy reference; and 3rdly, a handbook for assisting in the application of our resources to cases of disease. These requirements seem very simple and easily to be met, yet where can we point to anything like a full and uniform publication of our original records upon which practical homœopathy is built? Then as to the classified catalogue of symptoms, there is no complete repertory that commands more than a very partial confidence; and as to handbooks, though we have many, even in English, it must be confessed that we have to hunt through book after book to find that aggregate of practical information that might easily be contained in one volume. The originators of the Hahnemann Publishing Society conceived the idea of supplying all these wants, but the re-provings go on most tardily, the Repertory is incomplete, and the therapeutic part has yet to be begun. It was at first intended that a therapeutic chapter of clinical information should be added to each part of the Repertory as it appeared; but as that is far from completion, it is felt that the urgent demands of homœopathy call for an early publication of the clinical chapters in a

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separate form, constituting a manual of the practice of homœopathy brought up to the level of the present time. But we are met at the very outset by difficulties and objections. It is said that no work of the kind is properly required in homœopathy, as it leads to a contravention of its principles in attempting to prescribe for nosological entities instead of symptoms, whose proper place for reference is the Repertory; and practically having the vast array of symptoms of the Mat. Med. Pura before us, it looks like an attempt to compress the ocean into a gallon jar. As Clotar Müller says: "A homœopathic therapeutic manual, in the first place, can never include all the diseases met with in the daily practice of a medical man, even were it as perfect as possible, and contained all the diseases susceptible of being classed under any pathological system. The number of apparently unimportant cases which cannot be reduced to any diagnostic nomenclature is great, and their variety enormous. . . . . But even granted that we could get a therapeutic manual which comprised all diseases in easily found general titles, still, in the second place—and this is the chief thing—it could not contain more than a mere small selection compared with the inexhaustible varieties of each disease, and always only a circumscribed list of those medicines which might in reality be suitable." This is but a fair statement of the objections to such a work. But we are met, on the other hand, by the exigencies of practice, which call for an abridgment of the labour of consulting our repertories in every case. We are, therefore, obliged to trust largely to our memories, and would gladly turn to a book that might supply at a glance such useful hints as experience has stamped with its approval. We have but to look at our practice to see that experience counts for more than Hahnemann was willing to allow. It verifies pathogenetic indications, which is a great deal, and it gradually supplies the true interpretation of hosts of pure symptoms otherwise of doubtful meaning. The work under consideration is intended to garner the harvest of experience, and will thus become, as Dr. Bayes has happily expressed it, "the general practitioner," while the Repertory and Symptomatology must ever remain "the consulting physician."

The unsatisfactory nature of existing therapeutic manuals is due partly to the plan and partly to the

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manner in which they are worked out. We hope in the present attempt to improve upon both.

With a view to arrive at the best plan of the work, Dr. Bayes, Dr. Hughes, and Dr. Gibbs Blake published single specimen chapters in the *Monthly Review*, in order to elicit opinion, and as a result, at the Congress at York last year, Dr. Drysdale and Dr. Gibbs Blake were appointed to draw out a plan embodying the principles suggested in the above specimen chapters, viz., that it should consist of a therapeutical list of practical directions for the treatment of disease; 2ndly, that pathology, ætiology, diagnosis, and prognosis should be omitted, as information on these subjects is easily to be obtained in any good text-book on medicine; and 3rdly, that each chapter should consist of four sections.

Accordingly, a chapter on acute rheumatism was prepared and published in the *Monthly Review* for this month, and upon it we have a few remarks to make.

The first section gives the name and definition as suggested by the College of Physicians, London, in their recently published nomenclature, along with a list of medicines, arranged in three classes, the first class containing those medicines which have been discovered by the homœopathic law and tested in actual practice; in the second class are those medicines which are specific in their action, but only empirically known to be such, and which will probably be shown to be homœopathic; in the third class, those which, from their pathogenesis, would presumably be effective, but have not yet been tested by experience. The second section gives practical directions in treatment. The third section gives general treatment, diet, regimen, miscellaneous hints and bibliography; and the fourth section is an alphabetical summary of the medicines, accompanied with the clinical indications for each.

Now, as regards the first section, I think there can be no doubt of the advisability of eliminating the general information about diagnosis, prognosis, &c., as we do not want to consult such a book on these points, which besides rendering the book ponderous diffuses the knowledge we seek over a large space, instead of gathering it under our eyes, so as to be quickly glanced over.

As regards the classification of the medicines, I would very respectfully suggest that the three divisions should

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be resolved into two, the first class to include all those medicines which are of undoubted homœopathic specificity, and attested by actual practice; and the second class to include all others that have not the double qualification of undoubted specificity and practical confirmation. It is, I think, a confusing distinction to make two classes of those medicines that are only presumably homœopathic to a disease, whether they have or have not been tested.

Where simplicity of plan is so important, it is a great point to make the fewest possible divisions; and by a double class we gain this end, that we have the fullest confidence in those medicines in the first class, whilst those in the second are in a probationary state, and, in the course of experience will either be raised to the first class with full honours, or have their claims dismissed altogether; for our real wealth will consist in the numbers we can place in the first class. This point I would throw out for discussion.

The second section of the chapter is intended to be explanatory in adapting medicines to the various stages and incidents of disease. In the case of rheumatism, providing for the varieties of asthenic and sthenic attacks, the shifting and stationary forms, the complications, such as inflammation of the heart, brain, pleura, and lungs. Although the history of the disease is referred to, it is not for its own sake, but for the purpose of indicating those points of its tortuous course where special treatment is called for, which, in the case of prolonged and varying general diseases, will be extremely valuable in supplying us with a number of practical suggestions grouped together at the point where we should wish to find them.

I would venture one suggestion: that the morbid state referred to be always used as the text to discourse upon the treatment. This rule is followed in the early part of the section, but in the subsequent part the name of a medicine is used as a text to draw around it a number of therapeutical applications. In this way it becomes a repetition, both in form and substance, of what is said in the alphabetical list of the last section.

As to Section 3, it is devoted to the very important part of general treatment, including hygiene, regimen, the various hydropathic applications, mineral waters, electricity, shampooing, and the like, which often constitute an

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important part of the management of convalescence. The utility of this section commends itself without further argument.

The 4th Section, as before stated, is a list of the medicines arranged alphabetically for ready reference, and accompanied by the most pointed and trustworthy clinical indications possible. As a compendious, suggestive summary it will be indispensable. I have only a doubt as to whether its position in the chapter is the most desirable. I think the natural course of consulting the book would be to refer to the first list of authenticated medicines, when that refresher of the memory would often be sufficient, the previous knowledge supplying all other information at the hint of a name: or the second list might be consulted also for suggestions of probable help. If more light were sought in the choice of a medicine, the alphabetical list would be run over for clinical hints. But to do this readily the two sections should be placed together, whereas at present they are separated by two sections of discursive matter.

I would submit for discussion whether the present Sections 1 and 4 had not better rank as Sections 1 and 2. The tabular forms would thus be placed in juxtaposition, and the discursive matter would fitly close the chapter with desultory notes and bibliography.

As regards allopathic expedients, palliatives and the like, we must bear in mind that this work is not intended to be a summary of all known methods of treatment, but only of what is accredited in homœopathy; but I think such expedients may be mentioned as the imperfections of our art may require, by reason of its undeveloped state; or such as lie outside the sphere of homœopathy, and may be demanded for the welfare of our patients.

As regards the execution of the work, in order to obtain the greatest breadth of compass it is proposed that it shall not be the outcome of any single mind either in its totality or its several chapters, which is the fault of the best text books, allopathic and homœopathic. On the contrary, it is desired to enlist the greatest number of workers we can get. The organisation is not formed, but it is proposed to establish four or five centres, each consisting of several coadjutors, who shall undertake some section of the work and throw their combined knowledge into each chapter. When each centre has done its part, it shall be forwarded

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to each of the other centres for supplement; and finally, the whole work shall be revised by a competent committee, who shall undertake its publication at the lowest remunerative price. By this division of labour it is hoped the work will be expedited, and when once fairly launched the subsequent acquisitions of our art may be easily incorporated in new editions from time to time, and we may then hope to possess a handbook of practice which we can confidently refer to as containing the selectest homœopathic experience, and be able to place it in the hands of a convert with the assurance that it is an adequate expression of British homœopathy.

The outlines of the scheme are now before the meeting, and any suggestions that may be offered will be gladly received by those who are interested in its effectual accomplishment.

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PREFACE.

THE Annual Congress of Medical Men practising Homœopathy in Great Britain was held at the Homœopathic Hospital, Great Ormond Street, London, on the 4th of June, 1874.

On this occasion, five papers were offered for discussion, in addition to the *Address* by the President, Dr. DUDGEON. The time allotted for the business of the meeting did not allow of more than three of these papers being read. The discussions upon Dr. Dyce Brown's, Dr. Hale's, and Dr. Edward Blake's appears in the *Monthly Homœopathic Review* for June and July. Dr. W. B. A. Scott's paper on *Aloes*, and that of Dr. Mahendra Lal Sircar (which were unavoidably not read) appear in these *Transactions*, in accordance with the expressed wish of the members of Congress.

Dr. HAYWARD, the Secretary of the Hahnemann Publishing Society, read the report of the work done by that Society during the past year. This report,* which showed the flourishing condition of the Society, was regarded as eminently satisfactory.

A statement was also made, at the request of the President, by Dr. SCHNEIDER, of New York, explanatory of the objects aimed at in holding an International Convention of homœopathic practitioners in Philadelphia in 1876.

It was decided, by a large majority, that the next Congress should be held in Manchester, on Thursday, the 23rd day of September, 1875.

The proceedings were concluded by a dinner at the "Pall Mall," in Regent Street, when one hundred members and visitors were present.

The Congress met in the Board Room of the Hospital, an act of hospitality on the part of the Board of Management which was acknowledged by a hearty vote of thanks.

On the following page is given the list of Officers elected for the direction of the next meeting.

J. GIBBS BLAKE, } *Honorary*
ALFRED C. POPE, } *Secretaries.*

July 1874.

* Vide *Monthly Homœopathic Review*, June 1874, p. 458.

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THE INFLUENCE OF HOMŒOPATHY UPON GENERAL MEDICINE SINCE THE DEATH OF HAHNEMANN.

By R. E. Dudgeon, M.D.

Gentlemen,—Thirty-one years have elapsed since the death of Hahnemann, and I trust you will not consider it time misspent if I devote the hour allowed me for my presidential address to a rapid review of the principal changes that have taken place in the general practice of physic during this period, which may fairly be attributed to the influence of Hahnemann's teachings and the example of his disciples.

Up to the date of Hahnemann's death in 1843, it must be admitted that the effect of his doctrine and practice on the ordinary treatment of disease was inappreciable. The practice of medicine remained pretty much as it had been at the commencement of the present century. The abstraction of blood in acute inflammatory disorders by leeches, cupping and venesection was regarded by most orthodox practitioners not only as allowable but as indispensable. The depleting treatment attained its climax in the *coup sur coup* venesections of the illustrious but sanguinary Bouillaud. Purgation by the most drastic cathartics was regarded as the appropriate medication in almost every case. Mercurialization to the extent of salivation was still in vogue. Counter-irritation by blisters, rubefacients, issues, setons and the actual cautery was in daily use. Sudorifics, diuretics, emetics and so-called alterative treatment were universally employed. The great aim of the practitioner seemed to be to abstract his vital fluids from the unfortunate patient. The veins, the skin, the liver, the bowels, the kidneys and the glands were tapped simultaneously or in succession. As in the time of Molière, "seignare, purgare, clysterium donare" and all the allied forms of depletion were held to be the proper remedies for the most diverse diseases!

If, now and then, the observation of a better result

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obtained by the followers of Hahnemann led a stray practitioner to express a doubt respecting the propriety of the ordinary depleting methods, his scepticism was resented by his orthodox colleagues, and the crass humoralism that prevailed was irreconcilable with a method which professed to cure without the evacuation of any of the fluids of the body.

The great and learned Hufeland could not deny that brilliant cures were effected by the mild specifics of homœopathy; but, while admitting so much, he deprecated the adoption of this method, exclaiming that if it should prevail it would prove "the grave of science." It might be supposed that that science was scarcely worth keeping above ground which failed to cure patients as well as its unscientific rival.

In this country the eminent surgeon, Mr. Kingdon, gave mortal offence by extolling the anti-inflammatory virtues of aconite, which had been taught him by a disciple of Hahnemann. He predicted that the time would soon come when aconite would supersede venesection, and "our lancets would be allowed to rust in their cases." An outcry was raised at this heretical vaticination, and the medical periodical which takes its name from the maligned instrument poured all the vials of its wrath on the head of the rash author, who was fain to recant his heresy in order to escape more serious consequences. Since then lancets have everywhere been rusting in their cases to the unspeakable advantage of the patient-world.

The favourable, though limited, testimony to the success of homœopathic treatment given, with all candour, by the most eminent of modern surgeons, Mr. Liston, fell upon unwilling ears. Liston was too great a man to be bullied, and too courageous and independent to be made to retract a well-considered opinion, so he was let alone, and his testimony in favour of homœopathy quietly ignored.

So, in spite of the occasional protest of some distinguished member of the dominant school, the practice of that school remained uninfluenced by Hahnemann's teaching up to the time of his death. Every description of heroic practice (which, being interpreted, means violent interference with the normal processes) had its advocates. The most perturbing methods were held by physicians to be—what they euphemistically termed—their "sheet-anchors" in various affections. Thus, in inflammatory

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diseases, bleeding was the sheet-anchor of one set, tartar-emetic the sheet-anchor of another, calomel the sheet-anchor of a third, purgatives, sudorifics, vesicants, diuretics, the sheet-anchors of as many other sets. Other diseases had an equal or greater number of sheet-anchors. How such destructive processes ever came to be called "sheet-anchors" it would be hard to tell; or why "anchors" at all—"torpedoes" seems a more appropriate name for them, as they were designed to blow the disease to bits—but they just as often sent the patient to the bottom.

With faith so strong and so universal in these perturbing methods—which, moreover, harmonized with the gross humoral pathology of the day—it can scarcely be wondered at that the general body of practitioners received with incredulity the allegations of their brethren who had investigated Hahnemann's system, relative to the curative powers of medicines which exerted no disturbing influence on the patient's system, which drew off no fluids and excited no irritation of healthy parts. Impossible, they believed, to cure a disease without evacuating the *materies morbi*. If patients were alleged to be cured by such a preposterous method—one of three things—either the doctor had made an erroneous diagnosis, or he had given secretly some heroic remedy, or he had deliberately stated what was false. Thus, practitioners of homœopathy were credited by their orthodox brethren with being fools or knaves, or both.

There is a great deal to be said in excuse for this attitude of the ordinary medical man towards Hahnemann's system and its practitioners in the early days of homœopathy; for, after all, there is a great deal of human nature in doctors; and, touch their beliefs, or, as some would say, their prejudices, and they will behave pretty much as their non-medical fellow-creatures will do under similar circumstances. Now, in those days, the orthodox doctor had a belief. His *credo* might be formulated something in this fashion: "I believe in the remedial powers of bleeding, purging, puking, salivating and sweating." He had, as we know, no foundation for his belief—no evidence—only tradition; but we know that beliefs are not less stubbornly held because unsupported by evidence.

His belief was time-honoured, traditional, extending up through the great lights of the profession—Cullen, Boerhaave, Sydenham, Avicenna, Celsus, Galen, Hippocrates,

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Æsculapius—to the obscurity of pre-historic times, when men were taught by the dog how to give an emetic, by the hippopotamus how to bleed, by the sacred ibis how to administer a glyster.

He was so satisfied with the correctness of his belief that it never occurred to him to put it to the test of experiment. Or perhaps, like Don Quixote, with his patched helmet, he was so determined to believe in its soundness, but yet so doubtful if it would stand the test, that he deliberately refrained from proving it by a blow from the sword of experiment. He accordingly, like the Knight of La Mancha, took its soundness for granted. He would have deemed it the height of presumption to leave an inflammation without the bleeding, the blistering, the tartar emetic or other time-hallowed antiphlogistic treatment; and, if a medical coroner, he would unhesitatingly have directed the jury to return a verdict of wilful murder, or at the least of manslaughter, against any practitioner whose patient had died of inflammation unbled, unblistered, or unantimonialized.

With such strong convictions, it was inevitable that the orthodox practitioner should receive with incredulity the accounts of patients being treated successfully for serious illnesses by a method that produced absolutely no physiological effects on the patient, but only cured his disease without evacuations and without vicarious irritations.

He would believe anything tending to impeach the credit of the homœopathic practitioner, sooner than what would prove his own beliefs to be without foundation, and his practice—that practice transmitted to him through the greatest authorities of all ages—to be useless or worse.

Accordingly he was ready to credit every calumny brought against those who were seeking to overthrow his creed. And the writers of books and medical periodicals were not slow in supplying him with the material he was so willing to receive. Homœopathists, he was told, were unprincipled, dishonest, ignorant cheats and charlatans; though perhaps his fellow-students, and not undistinguished for their medical attainments at college, and their upright behaviour before they investigated homœopathy, he was told, and he professed to believe, that this investigation had caused them to become so ignorant that thenceforth they were unable to tell trivial complaints from

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serious diseases, or so dishonest that they would not scruple to represent slight disorders as the gravest maladies. The homœopathic system was caricatured and represented to be a monstrosity of folly and absurdity, which no one in his senses could believe, or could practise if he did believe it. All who prescribed medicines on homœopathic principles—nay, all who associated professionally with those who so prescribed, were held to be guilty of such flagrant immorality that they must be expelled from every medical society, and denied all professional intercourse with the orthodox. The courtesies and amenities of life were to be refused to any one who practised the hated system; the columns of the medical periodicals were to be closed to any reply to the frequent and scurrilous attacks made upon homœopathy and its practitioners; their advertising pages were not to be open to receive the announcement of any works, even on subjects apart from therapeutics, by those who acknowledged the principle of homœopathy as a guide to the treatment of disease. The examining boards of the colleges were converted into inquisitorial Star-chambers, and their diplomas were denied to candidates suspected of homœopathic leanings or who refused to promise never to practise homœopathy. Unworthy intrigues were set on foot to oust Henderson from his chairs of clinical medicine and pathology. Distinguished physicians were summarily expelled from hospitals they had served with honour and success, because they sought to give their patients the benefit of homœopathic treatment. Laws were passed by colleges and societies denouncing all who prescribed medicines according to the principle of similars as unworthy of association with those who prescribed differently. A medical bill was introduced into parliament, one main object of which was to put down what was called "irregular practice"—meaning thereby homœopathy. This object was fortunately frustrated by the vigilance of the practitioners aimed at, but the design was not the less apparent. Inquests held on the bodies of patients who had died under the care of homœopathic practitioners—for they would occasionally be so perverse—resulted in the finding by jurors under the direction of medical coroners of verdicts of "manslaughter;" at least, in one notorious case this verdict was, by the direction of Coroner Wakley, the brilliant editor of the trenchant *Lancet*,

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or rather, I should say, by that of his son and deputy, recorded against a practitioner whose own brother had died of Asiatic cholera while nominally under his care—but not really so—for, when the case took an unfavourable turn, the unfortunate practitioner was himself prostrated by an attack of cholera. It was, in truth, a glorious triumph of orthodox physic when the culprit was consigned to a cell in Newgate to await his trial for his life, nominally for killing his brother, but actually for his therapeutic heresies. To relieve the minds of those of you who do not remember the facts of the case, I may mention that on this occasion the majesty of "crowner's quest law" was not vindicated by the culprit's ignominious death on the scaffold, but that he was speedily released by a grand jury, which thus showed itself devoid of all proper feeling for the maintenance of orthodoxy in medicine.

I have recalled these circumstances to your recollection in order to show you that a conviction of the efficacy of a certain therapeutic method was stigmatized and punished as though it had been a felony, and this, not because this method was proved to be disadvantageous to the sick, but simply because it differed in some important features from the methods generally professed. The only evidence available showed that it was much more successful in its results than the ordinary treatment.

I may remind you of an incident illustrative of my assertion that the hostility of the old school was not owing to the failure, but apparently rather to the uncommon success of the homœopathic treatment. It made a profound impression at the time, and gave occasion to the writing of one of the wittiest and most humorously satirical articles ever penned against the attitude of the old school towards homœopathy, by that most profound of modern mathematicians, the late Augustus de Morgan, which you will find in the 13th vol. of the *British Journal of Homœopathy*. When, after the cholera epidemic of 1855, the House of Commons ordered a return of all cases of cholera treated in the London Hospitals, the College of Physicians, to whom was assigned the task of collecting these statistics, deliberately refused to admit the returns from the London Homœopathic Hospital—then in Golden Square—into their report, on the ground that these returns "would compromise the value and

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utility of their "—*i.e.*, the College's—"averages of cure;" the fact being that, while the mortality in the hospitals patronized by the College was 36 per cent., that of the London Homœopathic Hospital was under 16½ per cent. The cases admitted into the London Homœopathic Hospital were certified by the Medical Inspector of the Board of Health—who declared that he was prepossessed against homœopathy when he began his work—as being "all true cases of cholera in the various stages of the disease, that he saw several cases that did well under the treatment, which, he had no hesitation in saying, would have sunk under any other."

But in spite of denunciations, persecutions, caricatures, calumnies, *suppressiones veri, suggestiones falsi*, and coroners' inquests, the practice we owe to the genius of Hahnemann gained daily in public estimation. In medical matters the public looks only to results, and this test applied to homœopathic practice showed it to be, in numerous cases, vastly more successful than the ordinary treatment.

The growing confidence of the patient-world in homœopathy, together with the brilliant successes obtained by the practice among their own circle of acquaintances, as well as in public hospitals—more particularly the Homœopathic Hospitals of Vienna, which were visited and reported on by delegates from their own body—gradually forced upon the dominant school a reconsideration of their own methods of treatment.

They could not, or would not, admit that homœopathy was a positive method of medical treatment. They held it to be purely negative, the treatment of disease without medicine. Its undoubted success, its marked superiority to the ordinary methods in many of the most serious diseases, insisted on by so many of their quondam patients, and testified to by the investigations of eminent men of their own school, such as Sir William Wilde, of Dublin, in his work on Austria, and Sir John Forbes in the *Medical Review*, forced on them the enquiry as to the real value of their own perturbing methods. "As homœopathy," they argued, "is a mere negative—a do-nothing—treatment, perhaps its greater success in the treatment of disease is owing to its abstaining from those heroic measures which we have hitherto considered our sheet-anchors."

They therefore began to try how diseases would fare

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without being interfered with by active treatment. They found that their patients recovered all the faster. One of their number, Dr. Dietl of Vienna, instituted a crucial experiment on a grand scale in a large hospital at Vienna, of which he was the physician. He selected pneumonia, or acute inflammation of the lungs, as his test disease. The first series of cases he treated by bleeding in the accredited manner; 20 per cent. of the patients died. The next series he treated *secundum artem* with tartar emetic; again 20 per cent. died. The third series he left to nature; 7 per cent. only died. The publication of the results of this carefully conducted experiment gave the *coup de grace* to the old heroic practice, and a tremendous fillip to the so-called expectant or let-alone system, which had hitherto been practised in holes and corners, so to speak, but which now became the almost universally acknowledged creed of allopathy.

Here was an explanation of the success of homœopathy! It was but the expectant method with a pretence of giving medicine! It may be thought that the old school would have felt some gratitude towards its young rival for teaching it to cure its patients better by ceasing to make use of practices which did more harm than good, and that orthodox practitioners would have shown some compunction for their treatment of their homœopathic colleagues. But those who think such a result should have followed only betray their ignorance of human nature, of which, as I before said, there is a great deal in medical man. It is easy enough to forgive those who *tell* us we are wrong, but very hard to pardon those who *prove* us to be so—so homœopathy was abused in medical circles more virulently than ever. The fearful dishonesty of its practitioners in pretending to give remedies when it was obvious that they did nothing of the sort, was the text of many a homily in the medical periodicals, and the paltry persecution of homœopathists by the denial of all professional courtesy was carried on as vigorously as before.

Still the fact remained that owing to the results obtained in homœopathic practice, the time-honoured perturbing treatment of the old school was almost entirely abandoned. Lancets were actually rusting in their cases, cuppers were forsaking their avocation for some more remunerative and haply less sanguinary trade, leeches were left to disport in their native marshes, or only kept in

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bottles for the sake of their barometrical properties, blister-beetles were no longer captured by sackfuls, or at most, only used by hairdressers and milliners for cosmetic and ornamental purposes, drastic purgatives remained unasked for on the chemist's shelf, and sudorifics and emetics were a mere drug in the market.

The whole practice of medicine was changed with remarkable rapidity. The mildest drugs were alone employed. Treatment became at once "restorative" and "supporting." Diseases were all weaknesses requiring strengthening treatment. Tonics, nutritious food, and alcohol in every form, as wine, stout, ale, and brandy, were the fashionable medicines.

The complete change that took place in pathological ideas no doubt helped to produce this revolution in medical practice. The investigations of physiologists and pathologists completely discredited the gross humoral pathology by which alone the treatment of disease by evacuants could be justified. The general acceptance of the doctrine of the dynamical nature of disease gave the death-blow to all those monstrous forms of evacuant treatment which owed their origin to an antiquated materialistic pathology.

But some who conformed to the change of practice that discarded all the heroic treatment of former times were not willing to admit that the vigorous treatment of previous years was erroneous. They would not allow that they and their illustrious predecessors had been altogether or at all wrong in their treatment of inflammatory diseases by the violent antiphlogistic methods of bleeding, blistering and the like. To admit the fallibility of themselves or their professional ancestors in the matter of therapeutics would be to derogate from the honour of the profession, and to bring it into contempt. Of course they were right in their present treatment of acute disease by mild restorative medication, but they were equally right when they formerly treated the same diseases by bleeding and other evacuant measures.

A sort of Œcumenical Council was held at the College of Physicians of Edinburgh, whose members issued a decree declaring that the Medical Faculty was now and had always been infallible and unchangeable, and that it was disease that had changed, not they. "Change of type of disease" was the shibboleth that was to reconcile

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the altered practice with the recognised infallibility of the faculty, that was to smooth all difficulties and efface all apparent inconsistencies.

It was actually gravely contended that, within a few years, inflammatory diseases had so completely changed their type, that, whereas patients labouring under them formerly required to be treated by depletion, they could not now stand debilitating treatment, but needed to be supported and strengthened by tonics and stimulants. In short, human nature, it was asserted, had so altered that the self-same disease which was formerly a proof of excessive strength demanding all sorts of weakening processes, was now an evidence of excessive weakness calling for all manner of strengthening remedies. It cannot but be a matter of just pride to a graduate of Edinburgh to reflect that this ingenious theory was first put forward by the professors of his *alma mater*.

This "change of type" notion was received with acclamation by medical writers and journalists, and at one time seemed on the point of being universally accepted as the true solution of the difficulty, and a ready method of accounting for the change of practice without admitting the influence of homœopathy in the matter. To any one disposed to be cynical, it was as ludicrous a sight to witness the wild enthusiasm of old and grave doctors over the revelation of this scientific mare's nest, and the intellectual capers they cut round the supposed dead body of homœopathy, as it must have been to his townsmen to see the great mathematician rushing naked through the streets, and shouting "eureka!" like a maniac. But there was an excuse for Archimedes which our allopathic opponents did not possess, viz., that his was a real discovery. It enabled him to detect the fraud of the king's goldsmith; but the imaginary discovery of Alison and Co. did not enable them to convict their homœopathic brethren of deceit.

The chorus of mutual congratulations on the wonderful discovery had scarcely subsided, when the whole theory was upset by a few unenthusiastic sceptics, who pertinently—or as some thought *impertinently*—asked for proofs of the alleged change of type of disease. These were, of course, not forthcoming, so that the talk about "change of type" quickly subsided into ignominious oblivion, and its authors found themselves covered with ridicule, in place of glory.

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The fact is, as has been fully pointed out by members of our school, an inflammatory disease, such as acute inflammation of the lungs, though an excellent subject for demonstrating the pernicious effects of the depleting treatment, contrasted with a purely negative treatment, is not so well adapted for showing the positive value of the homœopathic treatment. If not improperly interfered with, pneumonia is by no means a very fatal disease, and a comparison of the results under the expectant system and under homœopathic treatment offers nothing like the disparity to be found in a comparison of the expectant with the depleting system. The difference between 20 per cent., the mortality under the depleting plan, and 7 per cent. that under the expectant method, is much more striking than the latter per centage, and 4 per cent., the mortality under homœopathic treatment. Henderson pointed out that homœopathy possessed the additional advantage of curtailing the duration of the disease. Still, the superiority of the homœopathic, over expectant treatment in pneumonia was only obvious after careful investigation. When merely the comparative mortality of the two systems was regarded, it seemed as though there was but little difference between the two, certainly not nearly so great an advantage of homœopathy over expectancy as of expectancy over the ordinary depleting treatment. Hence it was scarcely an unnatural inference that the superiority of homœopathy over ordinary treatment was mainly attributable to the same cause that accounted for the better results of the expectant treatment, viz.: the non-interference with the disease by depleting and other debilitating procedures.

The experience of Dietl in pneumonia, repeated as it was in many different quarters, and extended with like striking results to other inflammatory diseases, created an unduly favourable opinion of the expectant or do-nothing treatment of all diseases, and for a long time the leaders of medical opinion contended that all active medicinal treatment should be discarded, and that the sole duty of the physician should be to remove from the patient everything that was likely to interfere with the natural course of disease, to place him in the hygienic conditions most conducive to recovery, to attend to his diet and regimen, and to support his strength by generous food, and a more or less liberal use of alcoholic stimulants. In short, that the

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physician's chief duties should be a sort of combination of the offices of nurse and cook, and all medicinal interference was deprecated as being useless or even pernicious. In fact, medicine accurately corresponded to the sarcastic description of it given by Mephistophiles to the enquiring student, which we read in the first part of Goethe's *Faust*, and which I may freely paraphrase as follows:—

> The secret of med'cine is: Nature's vast forces
> You study in all their relations to man,
> And when you have done this, your only right course is
> To leave them to fight it out just as they can.

But it was impossible that such negative doctrines could obtain any wide-spread or permanent popularity among either the profession or the patient-world. The public naturally thought that the *raison d'être* of the physician was to *cure*, not merely to *nurse* disease. Medicines were justly held to be his curative instruments. The belief in the curative effects of medicines was too deeply implanted in men's minds to be annihilated in this fashion. Patients objected to having their diseases let alone. They had much faith in the remedial powers of drugs, and but little in those of nature, and they insisted on their doctors sharing their faith in physic. Some doctors who had lost their belief in medicines, had acquired a firm persuasion of the remedial virtues of stimulants, and, under the leadership of the late Dr. Todd, a system of treatment by alcoholic preparations was carried on, the consequences of which were in many cases most deplorable.

The school which arrogates to itself the titles of regular and orthodox, since it has discarded the traditions of its art and discredited the heroic practices so long in vogue, has devised no stable substitute for the treatment it has abandoned. The treatment by excessive alcoholic stimulation was unable to survive the distinguished man who was its ablest and principal exponent. In a modified form it still prevails here and there, but its professors are more cautious in its employment, and patients are acquiring a well-founded dread of it; so it may with confidence be predicted that it will soon be thoroughly discredited.

But now a new phase of medical treatment is coming into play. A faith in drugs is reviving, and though the progress of physiological and pathological knowledge forbids the revival of the old depleting and evacuant prac-

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tices, an active search is being made for remedies of another sort, and new "sheet-anchors" are taking the place of the old ones which went to the bottom in the disastrous shipwreck of traditional medicine. Expectancy which for a time, as we have seen, threatened to become the general rule is now denounced by those who lauded it. Indeed Harley, who is prominent among specific-seekers, declares expectancy as he declares homœopathy to be "the offspring of ignorance and deception."

There are still some who do not yet put any lively faith in the curative powers of drugs. Thus a learned lecturer, whose business it is to teach the Materia Medica to the rising generation of medical students at Guy's Hospital, the other day told his audience who had come to learn from him the curative properties of drugs, that the giving of medicines had just as much to do with the cure of diseases as the "oh-he-yoi!" of a gang of sailors has to do with the progress of the rope at which they are hauling.

Bromide of potassium at one time threatened to assume the character of a universal panacea, but is now rapidly going out of fashion. Chloral hydrate has almost superseded it, but already we observe signs of its waning popularity.

These are mere examples of fashionable medicines, employed for no special indications, but given like stimulants and tonics for merely general indications. It was, to be sure, at one time hoped that the first named salt would prove a specific for epilepsy, but even this hope has gradually died away.

Of late years, however, there has been a manifest tendency to search for real specifics, *i.e.*, medicines that produce their remedial effect without evacuation. The progress of physiological and pathological knowledge has tended more and more to upset the Sydenhamian doctrine, "that a disease is nothing more than an effort of nature, who strives with might and main to restore the health of the patient by the elimination of the morbific matter."—(*Sydenham's Works*, I. p. 29.)

The pathology of modern times has been gradually coming into accord with the homœopathic therapeutics. And this, I take it, is a remarkable and unique phenomenon in the history of medicine, that the therapeutics in consonance with a pathological theory should have pre-

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ceded the theory. It would not be far from the truth to say that the therapeutics in this case was the cause of the pathological theory. At all events, it is hard to imagine that the rational pathology of our times would ever have been invented or adopted by a race of believers in the virtues of bleeding and other evacuant treatment.

Hahnemann saw that the pathological theories of his time would not square with his therapeutics, so he was forced to reject them and substitute more appropriate pathological theories of his own. But Hahnemann, though a good observer was an indifferent theorist, and whilst his rule of treatment remains his pathological theories have for the most part fallen into oblivion.

Hufeland's prophecy has been fulfilled to a degree he little dreamt of, for homœopathy has proved "the grave of science"—of such science, pathological and therapeutical, as prevailed in his day, and could Hufeland see the state of medicine now-a-days, he would find the pathological theories and therapeutical methods he professed and practised all decently buried, and the master-spirits of medicine holding pathological views and pursuing pathological investigations in a way that must inevitably lead them, indeed has already led them partially, to adopt therapeutical practices in the sense of the principle promulgated by the illustrious contributor of so many original articles to his *Journal*.

He would see Trousseau practising homœopathy under the euphemistic title of "*médecine substitutive*." He would find Harley writing of belladonna that "its stimulant action is converted in febrile diseases into a tonic and sedative influence," and explaining this phenomenon thus: "Two similar effects, the one arising from a local irritation, and the other from the presence of belladonna, like spreading circles on a smooth sheet of water, interfere with and neutralize each other;" which is an explanation of homœopathic action that has been given in almost the same words by members of the school of Hahnemann. He would see Ringer choosing his remedies for the same indications as those laid down by Hahnemann. He would notice the repeated injunction by allopathic writers to prove medicines on the healthy in order to ascertain their remedial powers in disease, and he would find this advice occasionally acted on by eminent men of the orthodox persuasion. In short, he would observe that medical prac-

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tice was undergoing a complete revolution in the homœopathic sense.

There is no possibility of denying the fact that the chief medical authorities of the present day have for some time past been teaching that the physiological action of drugs must be studied in order to enable us to ascertain their therapeutic powers, and that medicines must be given which have a specific action on the self-same parts as the diseases for which they are remedial—this is the therapeutic principle, *similia similibus curantur*—in other words, homœopathy.

Most of you are familiar with the instances of this recommendation of the physiological proving of drugs in order to obtain a knowledge of their therapeutic uses, adduced by Henderson in his celebrated letter to Forbes. But these instances are stale. I will show you that the same plan is still advocated by partisans of the old school. The latest example you will find in the *Medical Record* of the 13th May last. Dr. Hull, of Philadelphia, there says, using the very expression introduced into the language by homœopathists: "When we proceed to investigate the therapeutic powers of a new agent, there are two general heads under which our studies should be conducted. 1. *Prove* its effects on a healthy organism. 2. After having determined its physiological action, proceed on general principles to elicit its remedial powers. A knowledge of drug action on the healthy organism enables us to form some idea of its adaptability to morbid states," &c. Unfortunately neither Dr. Hull nor his allopathic *confrères* who have seen the necessity of proving medicines and made attempts to do so, are content to give us a simple record of the effects observed. They must always overlay them with an attempt to give a physiological explanation of the symptoms—a laudable aim in itself, but for which their meagre provings afford them no sufficient data. They stiffen and glaze their simple material of provings, as the Manchester manufacturer faces his calico, with an elegant coating of pseudo-scientific devil's-dust, which does not add to its practical usefulness, for it is the genuine fibre of symptoms that is alone useful at the bedside, where the artificial gloss of physiological explanation is got rid of as soon as may be.

As for the treatment by homœopathic remedies by conspicuous members of the old school, what do my

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audience think of the following specimens of therapeutics, culled from the hospital practice of a distinguished lecturer on Materia Medica in University College and physician to University College Hospital, which were communicated to me by a student who had no knowledge or prejudice in favour of homœopathy? Half-drop doses of tincture of aconite in febrile disturbance, $\frac{1}{20}$th of a grain doses of corrosive sublimate for mucous and bloody stools, $\frac{1}{10}$th grain doses of hepar sulphuris for inflammation and threatened suppuration of the cervical glands, two-drop doses of the tincture of hamamelis for bleeding piles. Or what do they think of this advice given to his pupils by a lecturer on Medicine at St. Thomas's Hospital and a physician to the hospital? He told them, as the *British Medical Journal* informs us, that they were not "to allow prejudice to blind their eyes against certain remedial measures recommended by good authority, simply because they may have originated among homœopathists." Note the covert sneer implied in the antithesis of "good authority" and "homœopathists." It is as though a lecturer at the beginning of the century had said "do not allow prejudice to blind your eyes against the prophylactic virtues of vaccination recommended by many eminent London physicians, simply because the practice may have been originally proposed by an obscure provincial doctor."

While our medical brethren of the old school are thus inculcating and practising the methods and principles of Hahnemann, and while they are daily pilfering from his materia medica, they still pose themselves as our opponents. Whilst adopting the processes and instruments of homœopathy, they affect to despise and contemn it; they never mention the name of homœopathy without a sneer. They still exclude us from their societies, they still deny us professional intercourse, their organs in the press still misrepresent and caricature our practice. And yet all the while the columns of their journals teem with cases treated by homœopathic remedies which could only have been known to the authors from homœopathic sources. Provided these authors never mention the word homœopathy, or only mention it coupled with abuse, their homœopathic articles are commended, the writers are respectfully spoken of, they are honoured by the medical societies, they fill posts of dignity and emolument in the hospitals and col-

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leges, and are even appointed professors of therapeutics, while their chief glory is derived from their unacknowledged borrowed homœopathic plumes. Those who furtively avail themselves of the medicines given to us by Hahnemann's genius, while they openly revile the giver and continue to enjoy the emoluments and rewards of old physic, might fairly parody the words of Francis, and exclaim, "*Rien n'est perdu fors l'honneur!*"

But let a medical man give due credit to Hahnemann, let him truthfully confess that it is to homœopathy he is indebted for the remedies he finds so efficacious in disease; then at once all the medical periodicals of the dominant school are closed to him, medical societies exclude or expel him, he is deposed from his hospital appointments, deprived of his professorship, or, at the least, he loses all chance of obtaining any of the posts of honour that are generally supposed to be open to the profession and bestowed on the worthiest. In addition to this he loses professional caste and is regarded as unworthy of professional intercourse. Nay more, the Medical Directories refuse to publish the names of his medical writings or his appointments, and the medical periodicals deny him the right to advertise his works—even such as have no bearing on homœopathy—in their advertising pages. There are some conspicuous exceptions to the worst of these petty persecutions, but, as a rule, dishonest pilfering from homœopathy is applauded, while an honest avowal of obligations to homœopathy is vilified by the chief organs of medical opinion. Whatever may be the case in other walks of life, in the medical world as at present constituted, honesty is apparently *not* the best policy.

When homœopathy was new, and when the prevailing practice was still of the pernicious depleting character, and in fact so bad, that, as Sir John Forbes said, "it must either mend or end," it was easy to see that the partisans of old medicine would view the spread of homœopathy as fatal to their system, so it was natural that they should oppose it tooth and nail.

But now that the depleting system is almost universally abandoned; now that a more scientific pathology points to specifics of the homœopathic class as the most hopeful source of new remedies; now that the dominant school is daily enriching its materia medica by purloining from ours, it is difficult to see why the old antagonism to the school of Hahnemann is still kept up

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The honest though ignorant belief in the pernicious effects of homœopathic treatment of the practitioner of thirty years ago, has degenerated in his modern and more enlightened representative into a paltry trades-union jealousy, which encounters us not by arguments or experiments, but by the ignoble arts of detraction and calumny, accusing us at one time of letting our patients perish by withholding all remedial means, at another of killing them by adminstering virulent poisons.

I beg Dr. Moxon's pardon for asserting that our opponents do not now encounter us by arguments, for doubtless he considers the following passage, which I cull from a recent introductory lecture on Materia Medica and Therapeutics, as a smashing argument against homœopathy:—

"When like has any strictness at all, it means identical. It means that or nothing. Follow me carefully for a moment. A thing that is entirely like another thing is entirely identical with it. A thing that, without being entirely like another thing, is yet like it for some purposes sufficient for the meaning at the time, is *identical* so far as those purposes are concerned. In short, likeness is identity *so far as the word like has any force.* Stones of the same shape and size are alike for the mason; if they are of different durability, they are not alike for the house-owner. 'Yahoos' are like men for the purpose of being laughed at. Thus likeness may be defined as *identity for the purpose in question.* Yet the term like is, from boyhood, used so fast and loose, from the likeness of equilateral triangles to the likeness of Jack to a monkey, that, when the boy grows into a homœopath, he is ready to attach any meaning to like, from identity to the most fanciful resemblance. Now, in a general way, people believe that unlike things oppose each other, whether these unlike things are diseases or any other things. Such a notion is not worth much, unless you apply it strictly; but, in the only sense in which it has a strict application, it is an identical proposition; for you mean really opposite by unlike, or else you do not mean anything definable. You mean opposite so far as unlike; so that your proposition is that opposite things oppose each other so far as they are opposite—a proposition of infantile simplicity: true, surely, and cogent wherever applicable."

The learned lecturer's notions on the subject of "like" and "opposite" seem rather hazy. Probably it may assist his understanding of the matter if I draw an illustration of the two propositions from this very passage of his amusing lecture. If I say it is "very like" nonsense,

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and the "exact opposite" of a sound argument against homœopathy, I believe few will be disposed to disagree with me. The old nursery riddle, "What is likest to a cat looking out of a window?" with its answer, "A cat looking into a window," might have taught Dr. Moxon that likeness, so far from always involving the idea of identity, sometimes coexists with opposition. The confusion in the learned lecturer's mind on the subject of likeness reminds us of the intelligent African who declared Cæsar and Pompey to be "bof bery like, 'specially Pompey." The impossibility of answering such arguments seriously must be my excuse for departing from the grave style which ought to characterise your president's address. But even the wise Solomon admits that folly may sometimes be treated homœopathically. Dr. Moxon's arguments, however, are the wisdom of Solomon compared with the way in which homœopathy has always been treated in the medical societies, whenever the subject has been brought before them. All pretence of argument is eschewed, and the evening is passed agreeably in the recital of stories about homœopathy, of the "cock and bull" order. The latest instance of this is fresh in all your memories. It happened in the youngest of the metropolitan medical societies, the Clinical Society, which was established a few years back on superfine scientific principles. The members of this society had a lively discussion on homœopathy a short time ago, which, of course, as they were all of one mind on the subject discussed, they had all their own way. Each member tried to trump the narrative of the previous speaker by some more harrowing recital of the atrocities perpetrated in the name of homœopathy. Curiously enough the stories told were mutually contradictory. Thus, while one member stated that the homœopathic medicines contained large doses of poisonous drugs, the next speaker asserted that he had it on the best authority—that, namely, of a discharged assistant to a homœopathic chemist—that the globules and tinctures contained no medicine at all. He was followed by an eminent physician, Dr. A. P. Stewart, who gravely repeated the old story first promulgated some thirty years ago by the notorious travelling doctor for Bradshaw's Continental Guide, Dr. Edwin Lee—by the by, the youth on the side of the Tweed Dr. Stewart comes from, when they hear an improbable story, are in the

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habit of exclaiming, "That's a *lee!*"—but that is neither here nor there, though the same cannot be said of Dr. Lee, for he is here and there and everywhere. Well, Dr. Stewart came out with the great old Lee story of a certain Italian duke having died after swallowing three globules. There is the uncertainty usually observable in allopathic stories against homœopathy in the details of this case. The very name of the duke is variously stated as Canizzaro and Canigaro. The only thing certain about the matter is the three globules, which, being white, cannot be identified with the three black crows of another equally celebrated story. At the risk of divulging a secret that may hereafter be used against homœopathy, I will mention the real name of the party who fell an untimely victim to the three globules. It was no duke, not even an Italian, nor yet a man, but an English lady of the name of Harris, who may be heard of on application to a distinguished member of the sisterhood of monthly nurses, whose name I forbear to mention from motives of delicacy which I am sure you will appreciate.

What does this exclusion and professional excommunication of homœopathists mean? It means that a majority of the profession allege that some of their colleagues, who possess the same qualifications as themselves, who have been educated at the same schools and walked the same hospitals, are unworthy to be regarded as members of an honourable profession—are, in short, immoral individuals, with whom it would be ignominy to associate. And why? Because this excommunicated minority, taught by careful experiment, are convinced that many diseases are best treated by medicines which direct experiment shows are capable of acting on the same parts as are affected by the disease—a rule of practice which the majority only acknowledge in the case of a few diseases, as they have no experience either for or against the validity of the rule beyond these few diseases. The most exalted virtue could scarcely contend that there was aught of immorality in the belief that a great many—instead of only a few—diseases are best treated by medicines that act similarly to the morbific cause; and yet it is for so believing that we are treated by our colleagues in a so-called liberal profession as though we were guilty of some unpardonable moral delinquency. It may be all very well for non-professionals to laugh at this ridiculous persecution, but

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for us who are its victims it is no laughing matter. There is *odium theologicum* enough in the established church; but though the majority can call the minority bad names, they cannot deprive them of their places of emolument and honour. But the *odium medicum* of the established medical hierarchy, besides abusing, can very materially punish those whom it chooses to stigmatize as heretics.

E pur si muove! And yet homœopathy goes on and continues to modify and leaven the whole lump of general practice. The rising generation of medical men are continually being taught to cure diseases homœopathically by their medical instructors. The professor of Materia Medica in University College writes a highly lauded book on therapeutics, two-thirds of which are pure homœopathy. Black, Thorowgood and many others recommend bits of homœopathic practice without mentioning the hated word. Wilks filches from us while he abuses us; like Pistol munching his leek, "he eats and eke he swears." Harley pursues investigations into the action of medicines, and finding himself unexpectedly in agreement with the conclusions of homœopathists, deems it the correct thing then and there to denounce homœopathy as "the offspring of ignorance and deception."

At the rate things are going on at present, and under the guidance of his respected teachers, the medical practitioner will soon be practising nothing but homœopathy; but as his teachers carefully keep him in the dark, he will be practising it as M. Jourdain talked prose, without being at all aware of it.

The assimilation of modern practice to that of Hahnemann's followers is indeed admitted by one of the most recent lecturers on materia medica, Dr. Moxon, of Guy's Hospital, who says that "the practice of professors of homœopathy no longer differs from that of doctors who do not admit its absurdities." He means, of course, to imply that we have altered our practice in the direction of that of the orthodox school; but the inaccuracy of this statement scarcely requires to be pointed out. Is it we who have adopted bleeding, emetics, purgatives, diaphoretics, blisters, cauteries, salivation? or is it they who have abandoned these practices? Have we discontinued the search for specifics, or is it they who have begun to seek for them? Is there any one among us who could speak in such opprobrious terms of any of Hahnemann's thera-

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peutic teachings as Dr. Moxon speaks of the boasted "rational system?" He calls it the *deductive system*, and thus he inveighs against it:—

"The plan assumes that we know enough to act upon about the causes of disease. Gentlemen, it is difficult to speak in measured terms of the horrible results that have followed from this fatal assumption. It is this *irrational deduction* which has been the curse of medicine in all ages. This deduction, so irrational, is drawn from principles generally entirely false, and always so incomplete as to be beyond all bounds insufficient, so as to be worse than false in their effects, even if true in themselves, yet you hear some describing the deductive, or the so-called rational system as the pride of medicine. And those who have said this are so great, and so justified in feeling what the pride of medicine really is as a sensation, that one must submit; only replying, that if it be the pride of medicine, it has been the fatal curse of patients who have been hurried into the next world with the lancet or brandy-bottle on rational principles."

What is this but an amplification of Sir Astley Cooper's celebrated saying: "the art of medicine"—alluding, of course, to the allopathic art; he was not acquainted with homœopathy—"the art of medicine is founded on conjecture and improved by murder"?

After this fierce tirade against the rational system by a distinguished champion of that school which has so long arrogated to itself the title of "rational," we may bear with equanimity what he says about homœopathy, even when he raves about "the width and blackness of this disreputable blot on modern medicine."

Rational medicine being thus openly discarded with horror and contempt, and specific homœopathic treatment being furtively taught from the professorial chairs of the dominant school, and clandestinely practised in its hospitals, the general acceptance of our therapeutic principle must sooner or later inevitably follow; and we, who have consistently maintained that it is the true principle to guide us to the discovery of specific remedies, will then no longer be regarded as heretics, but as the pioneers of progress in medicine.

We have never accepted the position of sectarians which our opponents have endeavoured to impose on us, for in fact we have never been sectarians. We contend that we are simply physicians to whom the whole field of medicine is open, who are interested above all in the cure of

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disease, and who are ready and anxious to adopt every means whereby that object may be attained. In pursuing this object we have become convinced that medicines chosen according to Hahnemann's rule are the best remedies yet known for most diseases. If it can be shown that we are mistaken in this matter, and that there is some better rule for selecting remedies we are willing to accept it, but we refuse to give up our tried and valued rule for the less successful and lawless empiricism of our opponents.

If there are sectarians in medicine, the partisans of the dominant school rather deserve the appellation, who, while professing to avail themselves of the whole field of therapeutics for the cure of diseases, explicitly except that large class of remedies that act on the same organs and tissues as the morbific cause, though the efficacy of these remedies is testified to by the thousand-fold experience of large numbers of their medical colleagues, and many of their own favourite remedies are of this character whether they know it or not.

Hence we contend that we, and we only, are the true representatives of progressive medicine, and we protest against our exclusion by a sectarian and bigoted majority from those institutions and societies that were founded for the cultivation and improvement of the healing art, certainly not for stereotyping the prejudices of any medical sect.

We look forward with confidence to the good time coming when homœopathy shall be universally acknowledged as not only one of the recognized methods, but as the very best method of treating most acute and chronic diseases, where medicine is required, and when ignorance of homœopathic treatment will be held to be as disgraceful as a knowledge of it is at present deemed disreputable.

The medical art is not yet so perfect as to be able to dispense with the active co-operation of all its labourers. Quite recently an eminent authority of the old school has publicly declared that the present state of therapeutics is "deplorable." We agree with him, as regards the therapeutics of his school, and are ready to give the most satisfactory proof that the selection of remedies according to the principle of similars, renders the results of medical treatment considerably less "deplorable." Nothing but

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a false pride, owing to their having originally taken up a false position with regard to homœopathy prevents our brethren of the allopathic sect availing themselves of the means we offer them, and admitting our experience to be of equal value to their own.

We gladly avail ourselves of all the substantial improvements introduced into medical practice. We profit by the researches of the physiological school, and we hail with satisfaction the development of a more rational pathology, as they all tend to confirm the rationality of specific treatment. We employ with advantage the improved means of diagnosis afforded by the ophthalmoscope, the laryngoscope, the microscope, the stethoscope, the sphygmograph, the thermometer, and chemical analysis. Let our present opponents behave towards us with equal liberality and candour. Let them put our therapeutic method to the most stringent tests they can devise. We are content to abide by the result. If this should prove favourable, as we do not doubt, let them adopt so much of our practice as experience shall show to be good, and the existence of homœopathy as a separate school of medicine will cease. There will then be no allopathy and no homœopathy, but one medicine which will comprise all that is of approved value in therapeutics. There will be no homœopathists and no allopathists, but only medical men combating disease by the best known remedies. The homœopathic principle will be recognized as the rule whereby real curative specifics can be discovered, while the allopathic methods whereby some great physiological disturbance is effected will be assigned their secondary place as a make-shift practice, indispensable, no doubt, in some cases, to enable us to tide over a difficulty and clear the way for the specific curative treatment. We are at one with our old school colleagues in the great and important division of medical treatment, *hygiene*, which includes diet, regimen, and all sanitary conditions, why should there be variance among us in the other great division of medical treatment, *therapeutics ?*

We in this country who believe in the excellence of the homœopathic treatment of disease, have always acted as if we expected our colleagues of the old school to share our belief some time or other. It is by no wish of ours that the enmity that is apparent between the two schools exists. We have assumed no aggressive attitude towards estab-

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lished medicine. We have never sought to rear schools and colleges for the teaching of homœopathy as something apart from medicine, but have always looked forward to the adoption of our therapeutics by the existing schools and colleges. We have not flaunted our particular banner defiantly in the faces of our opponents. If we have set up periodicals with homœopathic titles, that was because the existing medical periodicals were closed to us, though open to all who chose to vituperate and calumniate us. If we have formed ourselves into homœopathic societies, that was because we were excluded and expelled from existing medical societies. If we have published homœopathic directories, that was because the existing medical directories refused to record our works and appointments. If we have established homœopathic hospitals, that was because we wished to show the success of homœopathic treatment to all enquirers, which can only be done in a hospital, and because the wards of the existing hospitals were shut against us. The small scale on which our hospitals are conducted shows that they were not meant as rivals to the existing hospitals.

We are not responsible for, and indeed have always set our faces against, the well-meant but injudicious attempts of enthusiastic lay adherents of homœopathy, who at its first introduction into this country sought to propagate it by methods admirably adapted no doubt for promoting the sale of an article of commerce, but which are less suitable for the propagation of a medical doctrine, and which doubtless increased the prejudice of the profession against homœopathy and its practitioners, though we, as I said before, and by "we" I mean all British homœopathic practitioners, were entirely innocent of these unprofessional proceedings and always protested against them.

The conciliatory and non-aggressive spirit in which homœopathy has always been advocated by its professional adherents in this country, and in every country of the old world, makes it appear that homœopathy on this side of the Atlantic is in a very stationary and dead-alive condition compared with its extremely demonstrative activity on the other side of the Atlantic.

While the number of professed practitioners of homœopathy in this country is under 300, the United States of America, with a population not much greater, has at least ten times as many homœopathic practitioners.

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While we have no schools or colleges for teaching homœopathy, America has several colleges entitled to confer degrees, and which do actually turn out many scores of graduates every year. While we have but one hospital of any considerable size, the one in whose hospitable board-room we are assembled, and which cannot make up much more than half a hundred beds, there are in America many hospitals for general diseases and several for special affections. Again, whilst this great metropolis can hardly raise £2000 by a bazaar for its homœopathic hospital—let us hope that the bazaar to be held this month may prove more productive—the comparatively small town of Boston in New England recently raised for a homœopathic hospital in that town by a bazaar the magnificent sum of more than £20,000.

What is the cause of this great difference? We are an eminently conservative race, preferring to *stare super vias antiquas*, unwilling to change our old fashions and our old laws—*nolumus leges Angliæ mutari*—and often sticking to the shadow after the substance has departed. This conservative feeling renders the public suspicious of new systems of treatment, and it has its influence on the methods we adopt for propagating the truths of homœopathy. We persist in considering ourselves as forming an integral part of general medicine, which is guilty of self-mutilation in excluding us; we assert our right to be admitted on equal terms to a share of all the honours and emoluments in the bestowal of the legally established medicine of the country, and we have no wish to set up any colleges or schools with sectarian appellations.

But it is otherwise in America. In that young country, where there are no old colleges, no time-honoured societies, no venerable periodicals, no hoary hospitals, where, if there is a preference in the national mind, it is in favour of what is novel—it was quite natural that homœopathy should set up its colleges and societies, its periodicals and hospitals side by side, and in distinct rivalry with the existing institutions of the old creed; for in that country one man is as good as another, and things may almost be said to be venerable in the inverse ratio of their ages. The novel system in America rapidly extended its branches in every direction, and enlisted at first starting a vast number of enthusiastic minds, who soon brought homœopathy almost up to an equality with the old school in the

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number of its adherents and in influence. But while we admire the real and substantial improvements and additions to our knowledge in Materia Medica and therapeutics that have been made by our translantic co-believers, we cannot but observe that the sectarian attitude assumed from the first by them has led some of them to run to extremes, which they would scarcely have done had they not cut themselves so completely adrift from the old vessel.

I do not allude to the eccentricities of some of our American brethren in any pharasaical spirit of affected superiority to such weaknesses. I only refer to them in order to point out that we have avoided such extremes by our adhesion to the idea that we belong of right to the established medicine of the country, and by having always endeavoured to act on the prudent maxim: "Behave towards your enemy as if you thought he might one day be your friend."

This spirit, which prevails not only in this country but throughout the old world, has no doubt its disadvantages. It has rendered us much less enthusiastic in working out the development and improvement of our special branch of therapeutics, in comparison with our American colleagues. While they have added scores of new, some of them well-proved and valuable remedies to the Materia Medica, our contributions in the same line are scarce worth mentioning. Our literature is insignificant in quantity compared with theirs, and we possess no means of training up youthful medical men in a knowledge of homœopathy.

A little more of the American zeal and industry would certainly be far from undesirable among ourselves. There is great danger of utter stagnation in the matter of the development of our therapeutics throughout Europe. Northern Germany, which when homœopathy was young assisted so materially in its growth, and furnished so many zealous workers at its development, seems to have subsided into a state of otiose contentment with things as they are, and makes but little effort to advance the system that took its rise in her midst. Austria made a noble effort to increase our knowledge of the action of drugs some thirty years ago, but for near a quarter of a century she has hardly given a sign of her vitality by any literary or scientific work, though during that time two Congresses have

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vainly tried to galvanize her into life. Hungary is the only country in Europe where homœopathy seems to be in a very vigorous state. With its two chairs in the Pesth University and its wards in the general hospital, it seems to occupy a position that should satisfy its partisans. Other countries of Europe seem to be pretty much as we are ourselves. Each has had its spasm of activity, and each has now subsided into the dulness of indifference or content.

But in spite of the absence of noisy demonstrativeness, I am convinced that in none of the countries of the old world is the real progress of homœopathy very far behind what it is in America. While the new converts of the States come out of the old ranks in an ostentatious manner and enrol themselves openly among the adherents of the new school, our new converts remain in the old ranks and are careful to avoid an open alliance with what is still denounced as a heresy. The Ringers, Harleys, Wilkses, Thorowgoods and Burnesses of America fill chairs in the homœopathic colleges; here they stick to the old craft and are rewarded by professorships and the applause of orthodox journalists.

The influence of homœopathy on the general practice of medicine since the death of Hahnemann may be divided into three stages. There was the stage of fierce defence of all the evacuant and debilitating processes which had existed almost unaltered from the earliest times, and which were in full swing thirty years ago. Old physic would not listen to a word against those time-honoured practices.

The next stage was that of utter unbelief in the remedial powers of drugs in general and of evacuant and depleting agencies in particular. The experience of homœopathy, backed by the results of Dietl's experiments, awoke the orthodox medical world from its fool's paradise of contentment with traditional medicine. The horror then excited at the sanguinary treatment that had hitherto prevailed, exists as strongly as ever among doctors and patients. Were anyone now to propose to revive the plan of bleeding *coup sur coup* in inflammations, which was practised within the memory of most of my audience by one of the foremost teachers of orthodox physic, he would be regarded by his colleagues as a lunatic, and by his patients as on a par as to medical knowledge with the

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medicine-man of the South Sea Islands, who professes to cure a headache by scraping a hole in his patient's skull with a bit of broken glass in order to let out the pain—a proceeding as capable of justification on rational principles as opening a vein to let out an inflammation. The feeble attempt of Richardson in recent times to revive blood-letting has been received with contempt by the profession, and even the *Lancet*—the very instrument of venesection—has not a word to say in its defence.

The very surgeons who used in former days to shed blood like water, and who thought, or at least said, that the blood a patient lost during an operation, rather did him good than harm—even they have been seized with a Mosaic respect for the blood, and now all go in for bloodless operations, repelling all the blood from the limb to be amputated into the patient's body before commencing their operation; so that the patient, after his amputation, has relatively more blood in him than before, for he has not lost a drop, and has a limb the less to supply.

The disrepute into which drugs and all active treatment fell gave a temporary impetus to expectant treatment or therapeutic nihilism. However, this merely negative system could not last. For a time it gave way to a vigorous revival of the Brunonian practice, and stimulants were everywhere the order of the day.

But now old physic has entered on another stage, in which expectancy is denounced as "the offspring of ignorance and deception," and a search is made for specific medicines, *i.e.*, medicines that have a physiological action on the same parts as the diseases they are to cure. Some, like Harley, make researches for themselves, and are disgusted at finding that they arrive at the same results as the homœopathists. Others, like Wilks, and Ringer, and Murchison, spare themselves the trouble of original experiments, and boldly dip into the homœopathic Materia Medica for their specifics. By this means Wilks has discovered the antiphlogistic properties of aconite, and Ringer the specific virtues of a host of medicines, the last of his discoveries being the remedial powers of hepar sulphuris in the purulent process. As Hahnemann gave his name to a preparation of mercury he discovered, which is called to this day *Mercurius solubilis Hahnemanni*, why should not Ringer have our *hepar sulphuris* renamed after himself, *Calcii sulphidum Ringeri*, for there can be no question

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about his singular merit in having discovered its virtues in the purulent process after painful and laborious search in the tenebrous depths of the Homœopathic Materia Medica? And as the Countess of Cinchona gave her name to the plant of antifebrile renown which she first drew from its obscurity among the ignorant natives of Peru, so might not Wilks set up a claim to have *aconite* rebaptized by the name of *Wilksia*, to commemorate his immortal discovery of its anti-inflammatory properties, when poking about among the farrago of drugs employed by the obscure practititioners of what he is pleased to term "an eminently quack system?" I rather fear that posterity will treat Wilks's pretensions to be the great medical authority on the antiphlogistic powers of aconite by quoting this line of Juvenal—

"Nulla aconita bibuntur fictilibus,"

which, I need hardly tell my colleagues of a learned profession that writes its prescriptions in Latin, means—

"We don't drink our aconite out of a Wilksian mug."

Whilst the practice of the dominant school consisted mainly of those perturbing operations, which had been the ordinary rule of practice for centuries—and again, when, taught by the experience of the homœopathic school, they had lost all faith in heroic treatment, and had drifted into expectancy and alcoholic stimulation, homœopathy spread with wonderful rapidity through the country, and made numerous converts among both doctors and patients, who had acquired a wholesome horror of violent treatment, and who had not lost faith in physic. In those days any practitioner who declared himself a follower of Hahnemann was sure of getting rapidly into a large practice. The mere material inducements to avow oneself a homœopathist were of the most tempting character, and homœopathy was so utterly unlike the treatment of the old school that one could not practise it and remain a nominal adherent of the dominant sect. Hence there was every inducement, and indeed a necessity, for those who were convinced of the truth of homœopathy to range themselves in the ranks of the professed followers of Hahnemann.

But since the partisans of established medicine have abandoned both heroic measures and expectancy, and

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busied themselves with the mild medication of specifics, there is no longer the same horror of their treatment among the patient-world, nor the same inducements to practitioners to associate themselves among the professed adherents of the homœopathic school. Indeed the inducements are all the other way. Large practices are not now at once obtained by the profession of homœopathy, and a declaration of adhesion to Hahnemann's therapeutics cuts a medical man off from the honours and emoluments that are in the gift of the old school. Besides, one can now practise pure homœopathy within the bosom of the old school without exciting any opposition on the part of old-school colleagues—indeed, with the applause of old-school journalists, and without disqualifying oneself for professorial chairs and hospital appointments.

Such being the case, it is not to be wondered at that we now obtain few open accessions to our ranks; that the vacancies occasioned by deaths are scarcely filled up by new converts. Apparently homœopathy is almost at a standstill as regards professed adherents in this country. The *Homœopathic Directory* of this year contains few new names of practitioners that were not in the *Directory* of last year. Nor is it surprising that we have renegades, for there are some who are more influenced by what they deem their own interest than by zeal for science and a desire to render justice to the illustrious but unpopular founder of the improved therapeutics. If by a simple renunciation of their connexion with the homœopathic school they can hope to share in the loaves and fishes of established medicine, while they continue to practise the better therapeutics, the temptation to commit this act of moral cowardice is too strong to be resisted. These weak-backed individuals are, to use an Americanism, the "carpet-baggers" of medicine, who, with their meagre modicum of intellectual luggage, squat down on the territory that has been conquered without any aid from them; and when they have obtained their ends, they betake themselves, with their carpet-bags well stuffed with "notions" acquired during their sojourn among us, to "fresh fields and pastures new."

The paucity of recent conversions to homœopathy in other countries of Europe has been admitted and commented on by the homœopathic periodicals, and is, I think, satisfactorily accounted for by what I have stated. The

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prospect of obtaining a large practice by the profession of homœopathic treatment is problematical and remote, while the disadvantages of loss of professional status and exclusion from all the great prizes of the medical profession are only too certain and near; so the young practitioner, whatever may be his convictions of the superiority of homœopathy, feels that he would be too heavily handicapped in the race for professional distinction by declaring his convictions.

Hence we cannot now look for any great increase of the numbers of avowed followers of Hahnemann, but must rest content with the assurance that the principles of homœopathic therapeutics are being largely adopted and acted on by nominal adherents of the old school. In fact, the boundary line between homœopathic and orthodox practice is daily becoming less distinct. As in ancient times there were tribes of men living on the border-land betwixt England and Scotland who committed themselves to no professions of nationality, but as it suited them pillaged either side with perfect impartiality; so on the borderland of allopathy and homœopathy there dwell many who without professing any particular creed indulge in the practice of "requisitioning" at one time from the homœopathic, at another from the allopathic, Materia Medica for the remedies they employ. As the Union of England and Scotland extinguished the border freebooters, so our medical borderers will be extirpated when old and new physic are united in the common cause of true medical progress.

We may marvel at the disingenuousness of those who appropriate the fruits of the labours of Hahnemann and his followers without acknowledgment, but we know the cause of this. As yet they cannot act on the maxim *suum cuique tribuere* without incurring losses they have not the courage to face. We may quote "*sic vos non vobis*" for our consolation, and hope for the speedy advent of the day, when those who, under the guidance of teachers who well knew the source of their inspiration, are employing the remedies of our pharmacopœia in the treatment of disease, *sans qu'ils en sachent rien*, will demand of their teachers their authorities for the alleged actions physiological and therapeutical of these new drugs, as the *Dublin Medical Journal* lately enquired of Sydney Ringer for his authorities; and when they learn the truth, we have sufficient

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confidence in the love of justice of our colleagues to believe that they will give to homœopathists the credit they deserve, and where they have been so long endeavouring to rear a Tyburn gallows for our punishment, they will wish to erect a Marble Arch to our honour.

Though the flow of avowed medical converts to homœopathy seems to be diminished or almost arrested, the conversions are not less numerous than in past years, only they are not avowed, and the universal prevalence of the system is assured. The old school still reviles us, while adopting our methods and remedies; she still asserts her exclusive possession of the truth in medicine, while abandoning one by one all her traditional beliefs and practices.

As Cambronne at Waterloo exclaimed, "la Garde meurt mais ne se rend pas!" (or something equivalent), but thought better of it and surrendered, so allopathy, on the eve of her surrender, sings, "Britons never, never, never will be homœopaths," threatens to nail her colours to the mast and rather go to the bottom shouting "Vive l'allopathie!" than yield an inch of her territory or a stone of her fortresses to the advancing conqueror, who has already beaten her out of all her strong places, stormed her Strasburg of bleeding, overwhelmed her with confusion at her Sedan of salivation, battered down her Metz—or as the French pronounce it her *Mess*—of blisters, cauteries and emetics, and now closely invests her in her last fastness, which is already distracted by intestine dissensions, and whose much vaunted citadel the Mont Valérien of rationality, last hope of orthodoxy, has just been basely given up by the traitor Moxon.

Until the leaders of medical opinion in the dominant school shall have ceased their illogical and insincere opposition to us, and shall have admitted our claim to have the homœopathic method recognized as a legitimate mode of ascertaining the remedial virtues of drugs, of value at least equal to their own empirical no-method, our duty clearly is to cultivate our own field of therapeutics diligently, to increase and perfect our provings, and to render them more available in practice. When opportunity and leisure offer we should not neglect the other branches of medical science, and the singularly valuable and profound essays of Henderson, Drysdale, Madden, and my immediate predecessor in this chair, Sharp, on physiology and pathology, the important discoveries of Drysdale in the

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life history of the minute organisms revealed by the microscope, and the masterly work of Blackley on the origin of hay-fever, a work which has obtained the well-merited applause of all the organs of medical and scientific opinion, prove that a profound acquaintance with homœopathic therapeutics and skill in their application, by no means disqualify their possessor for attaining the very highest eminence in physiological and pathological science.

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ON THE ACTION OF NITRIC ACID IN CERTAIN FORMS OF COUGH.

By D. Dyce Brown, M.A., M.D.

If there is a great charm in opening a new mine of wealth in medicine, by discovering a new drug, and pointing out its indications for treatment, there is an almost equal charm in bringing into prominent notice a medicine, which, though long known, is little used, and especially in developing its value in certain particular forms of disease, in which, to judge by the rarity of its prescription and notice in our journals, it is almost unknown.

One is very apt in the everyday round of practice to prescribe our well known and well tried medicines, and to allow the virtues of other drugs, equally valuable in their own spheres, to be forgotten. In this present paper, then, I please myself by the thought that I shall perhaps render a small service to my homœopathic *confrères*, by endeavouring to develope the value of nitric acid in certain forms of cough. Of course there are other uses of nitric acid, but it would make much too lengthy a paper to take up more than the one point I have named. I shall break this resolution in only one particular, by noticing its great value in constipation. This is a point of which I was not previously aware, but which forced itself upon my observation in the treatment of the cough cases.

The only full account of the pathogenesis of nitric acid is in Hahnemann's *Chronic Diseases*, vol. iv., where it is classed among the anti-psoric remedies. Dr. Hughes, in his *Pharmaco-Dynamics*, says of this pathogenesis, "I must confess myself at a loss what to make of the provings in that remarkable work. Until the day books of the provers are published, and the quantity and frequency of the doses taken are ascertained, I feel the utmost un-

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certainty as to the reality of the numerous and multifarious symptoms ascribed to the 'antipsoric' medicines. With nitric as with muriatic acid, I must direct you for the present to clinical experience as the only available means of ascertaining its sphere and mode of action."

With all due deference to Dr. Hughes, I think this is rather a severe criticism on Hahnemann's pathogenesis. Of course the meaning and value of all the recorded symptoms would be much more apparent, had the particulars of the doses been given, but still I think we can see running all through the proving a state of body, which in my experience is the one where nitric acid is chiefly indicated. It is, to state it shortly, a state of general physical depression, which shows itself in various ways. In a paper like the present it would be impossible to analyse and comment upon all the morbid conditions described as present by Hahnemann, but in the majority of cases where I have found nitric acid indicated for cough, and of service in its removal, there is this general state of physical depression more or less marked. Perhaps the most interesting method to pursue, instead of giving a list of pathogenetic symptoms, will be to draw a picture of what in my view is a typical nitric acid case.

The complaint is, to begin with, an essentially chronic one. All the symptoms point to this; and I do not think that it is at all a medicine suited to acute cases. The patient has from the continuance of the disease got into a general state of bad health. There is general lassitude and weakness, with loss of energy, feeling of unfitness for exertion and work of any kind, and after any work or exertion, a state of unusual and abnormal tiredness. With this there is more or less mental depression, which is simply the result of the physical weakness. There are occasional headaches, which seem to me the result of a part of the disordered state of the digestive organs. The digestive organs show also this condition of depression. There is generally no gastric irritation, in the proper sense of the word, but a want of appetite, associated with a tongue which may be quite clean, or slightly coated towards the back; a bad taste in the mouth in the morning; and after food a feeling of fulness or distension, often actual pain, and a tendency to be squeamish. Hahnemann mentions both a state of constipation and of diarrhœa as present. My experience is that con-

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stipation is the prevailing state, often to such a degree as to induce the patient to take purgative medicine for its relief. There is generally considerable loss of flesh, unrefreshing sleep at night, feeling of feverishness at night, with hot skin and thirst, and often night-sweats. In women leucorrhœa may be present. Then there is generally cough in several different forms. These I would divide into two principal kinds, the non-phthisical cough, and the phthisical. In the former class of cases, on examination of the chest, we find little abnormal. No dulness on percussion, no tubular breathing, nor increased vocal resonance, nor even bronchitic sounds; perhaps feebleness of breathing, and sometimes not even that. The form of the cough in these cases is one coming on chiefly during the day, though it also may cause some trouble on first lying down in bed. The cough is sometimes nearly dry, what expectoration there is being mucous and rather difficult to bring up; at other times there may be a considerable amount of sputum; the cough may be a short cough, or may take the form of "fits" of coughing, in which case it may sometimes cause retching, as in hooping cough. There is frequently, but not always in such cases, more or less uneasiness in the chest, soreness at the bottom of the sternum, or localised pains in either side of a sharp or dull character. Another very common variety of cough is that where it occurs almost entirely in the morning, on first waking, or on getting up out of bed. There is then a good deal of coughing, with a considerable quantity of mucous expectoration, after which during the day there is nothing more than an occasional cough till the time of going to bed, when for a time an increase of the coughing occurs. In these cases there is little or no chest pain, and less disturbance of the general health than in the former cases. A variety of the latter class of cases is, when along with the morning cough, there is a considerable amount of shortness of breath, which also passes off after the morning expectoration is brought up, or after a nervine stimulant, such as a cup of tea. Another class of cases in which nitric acid is frequently of service is in cases of emphysema. In such cases, with the usual physical signs of emphysema, we have shortness of breathing on any exertion, and especially on going up a stair, considerable paroxysms of coughing, with dyspnœa early in the morning, and also

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a considerable amount of mucous or muco-purulent expectoration. Arsenic here and ipecacuan are of great value, the latter where the emphysema is accompanied by a chronic form of bronchitis, as shown by the presence of dry râles in the apices and moist râles at the bases of the lungs, and the former where there is not this accompaniment. But in both these forms of the complaint, we often find the patient present a state of general dishealth, such as I described as the typical nitric acid state. Here we find that the acid does as much good or more than arsenic, not only in improving the general health, but in a very decided manner improving the condition of the chest symptoms.

There is one other set of non-phthisical cases, which are probably the ones alluded to by those few writers who have mentioned the use of nitric acid in cough. Dr. Hughes, for example, in his article on Nitric Acid in his *Pharmaco-Dynamics*, says, "In the respiratory tract it controls the nasal and laryngeal mucous membranes being curative of .... dry and violent laryngeal coughs." Dr. Bayes, again, in his work on *Specific Restorative Medicine*, describes a cough, which is unmistakably laryngeal. He says (p. 124), "Another affection in which nitric acid 3rd has proved very serviceable is in a chronic laryngeal cough, without expectoration, which is characterized by a stinging or smarting sensation, as if a small ulcer were there, and is generally felt on one side. The 3rd dilution of the medicine often speedily arrests and cures this cough." Of course the local pain shows that such a cough is laryngeal. But the cases to which I now allude are not so clearly laryngeal. I have had several such where a teazing short dry cough occurs, and lasts for a long time. Two such cases I shall give afterwards where this form of cough lasted for weeks, until nitric acid was prescribed, under the use of which it rapidly vanished. In these cases there was no pain in the chest, nothing whatever abnormal to be discovered on examination of the chest, and no disorder of the general health. There were also no symptoms referrible to the larynx, no hoarseness, soreness on coughing, or tenderness on pressure, nothing in fact but the long standing short dry cough, which continued all day, was very troublesome on first lying down at night, keeping the patient awake for some time, but either not coming on at

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all after sleep once supervened, or so slightly as never to waken the patient. What makes me hesitate in calling this a laryngeal cough, is not only the absence of all laryngeal symptoms proper, but the entire failure of all medicines usually successful in laryngeal disorders. I myself think they were not laryngeal, but were simply a manifestation of a susceptible condition of bronchial mucous membrane, as in the two cases referred to, the same cough returned every winter for two or three years, and in a slighter form, if there was any sudden change to cold weather.

Before leaving the non-phthisical cases, I ought, to make the subject complete, to notice the use of nitric acid in hooping cough. But this only requires a passing notice. It has been recommended by several allopaths, but the doses prescribed have been so large—20 to 30 drops of the dilute acid of the pharmacopœia, which is the same strength as our 1x dilution, and this for young children—that I for one could never think myself justified in pursuing this course of treatment. The fact of these doses being given so much larger than those usually given to children even by allopaths, would show that they had found smaller doses not answer. I have tried the dose I usually give in other cough cases once or twice, with no beneficial result. I therefore propose to pass on with these few remarks to the *Phthisical cases.*

I think that most physicians now admit the truth of Niemeyer's views, that there are two forms of phthisis, tubercular and non-tubercular. In cases that are unmistakably those of acute tuberculosis, no treatment that I am aware of does any good. They go on their fatal progress steadily in spite of homœopathic or any other remedies. But it is very different with cases which have begun primarily as pneumonia, the lymph in which has degenerated into the yellow cheesy deposit so frequently found at post mortem examinations. When these cases show active disease going on, as evidenced by rapid pulse and high temperature, nitric acid is not indicated. But when we have subdued the acute stage by other treatment, and the patient has a nearly normal pulse, and a normal temperature, but when on examination the physical signs of phthisis are present, with a state of general weakness, loss of flesh, nocturnal perspirations, bad appetite, and sluggish bowels, with a troublesome cough and

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a good deal of muco-purulent expectoration, both cough and expectoration being worst in the morning and on lying down at night, then nitric acid comes in with gratifying results. The general health improves, a renewed feeling of strength is felt, more relish for food, and ability to take more of it follow, the bowels become regular, and both cough and expectoration are wonderfully lessened, till they are reduced to a minimum. I feel sure that any one who uses nitric acid in such cases will agree with me in considering it a very valuable addition to our *armamentarium*.

In conclusion, as I stated at the commencement of this paper, I shall say a few words upon the value of nitric acid in constipation, my apology for so doing being that it comes in naturally as a comment upon the cases I am going to relate. I was not previously aware of this feature in the therapeutics of nitric acid, and so far as I am aware, it has never been noticed in any of our Journals.

Hahnemann in fact at the very commencement of his article on Nitric Acid in the *Chronic Diseases*, says, "It is, moreover, more suitable to those chronic patients who are disposed to looseness; it is very seldom useful to those who suffer with constipation;" and yet in spite of this statement we find under the head of "Stool" the following symptoms:—

"Constipation, painless constipation for several days. Every other day hard stool covered with mucus, during the first days, afterwards every day; costiveness; the abdomen became distended, but the flatulence did not pass. Hard, scanty stool. The stool passes off in hard lumps. Stool resembling sheep's dung, with much pressing, and accompanied with mucus. Pressure upon the rectum, as if stool would come on, with scanty discharge. Long pressing when going to stool; the stool would not come off, and nevertheless it was not hard. Constant desire for stool, unsuccessful."

One wonders, after being aware of these symptoms, that Hahnemann should have made the statement I have quoted. I gave constipation as one of the frequent symptoms present in what I called a typical nitric acid case, and almost invariably, along with the improvement in the other symptoms, the bowels become regular and easy. So marked was this result, that I have several

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times prescribed the acid as a remedy for constipation, independently of any other state, and I have found it successful, after the failure of the other usual remedies.

I have no hesitation in placing it in the foremost rank in our medicines for constipation, and I am sure that any who follow my recommendation, and try it, will be well satisfied with the results.

Finally, as to the dose, I have always given two or three drops of the 1x dilution. Possibly a less dose might be successful, but I have found such gratifying results both in this and in the cough cases, that I feel inclined to keep to my present dose. If others who try the medicine in the cases I have described, find higher dilutions equally satisfactory, I should feel obliged by their recording the facts, but on the other hand, if higher dilutions do not produce the expected result, I ask in fairness to me, to the medicine, and to themselves, that they make use of the 1x dilution, in two or three drop doses, three times a day.

I am afraid that I have already consumed the time allotted for each paper, but if not wearying the members of the Congress, I shall give a few cases in illustration of the position I have taken up.

Case I.—Mrs. ——, æt. 50. Had for several weeks been very much out of health. She was housekeeper in a large household, and of late had felt quite unfit for her duties. She complained of feeling tired with the least exertion; even going about the house fatigued her. She had lost flesh, slept badly at night, and had nocturnal perspirations. Her appetite was bad, and she had feeling of indigestion after eating. The tongue was clean, and the bowels very costive. Her pulse was quiet, and temperature normal. Catamenia had ceased several years. She had besides a severe cough, chiefly in the morning, when she brought up some muco-purulent expectoration. Through the rest of the day she had the cough only occasionally, though at night, on lying down, it rather troubled her. She complained of pain across both apices of the lungs. On examination of the chest nothing unusual was found, except that the breathing was feeble. She was greatly depressed in spirits, and thought herself dying. I prescribed nitric acid 1x, 3 drops three times a day, in a wineglassful of water. I saw her again a fortnight after. There was then a great improvement. The cough was very much better, in fact nearly gone. The chest pain had gone. Her appetite had become good, the pain after food gone, and the bowels had been acting regularly.

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Her appearance was much better, her strength improved, and her spirits much better. I continued the acid, and in a short time the cough disappeared, and she was otherwise well.

Case II.—Mrs. F., a confectioner, æt. 35, being subject in her occupation to changes of temperature, was apt to take cold, and cough occasionally. Before I saw her she had been ailing for a fortnight with a severe cough, which was equally bad by day and night. The cough came in paroxysms, and frequently caused retching. A good deal of mucous expectoration was coughed up. There was pain over the sternum when coughing. The cough kept her awake at night. She had been losing flesh, had no appetite, the food gave her pain in the stomach. The tongue was clean, bowels very costive, pulse quick and small. Temp. normal. Had much thirst. Catamenia regular. On examination of the chest I found nothing abnormal, except feebleness of breathing over both lungs. At night she had a good deal of perspiration. She complained of great weakness, and inability to do anything in her house, with shortness of breathing on going up a stair. She was also depressed in spirits. She was kept in bed for some time, and had aconite, ipecacuan, antimony, and arsenic for medicines, for about a month. At the end of the time she was in the same state, except that she was thinner and weaker, and latterly unable to rise from bed. The pulse was still quick, though the temperature was normal. I made repeated examinations of the chest, but could discover nothing fresh. I then prescribed nitric acid in the usual dose. At the end of a week there was a decided improvement in every way. In another week the cough was almost gone. She slept well, and the night sweats had nearly stopped. The appetite was improved, pain after eating gone. The bowels were acting regularly. Pulse normal. Feeling of general strength much better. The medicine was continued another week, by the end of which time she had no cough, and was quite well, and able to pursue her calling.

Case III.—E. F., a lady's maid, æt. 30. Several members of her family had died of consumption. For two months before I saw her she had had a cough in the morning and on lying down at night. Not much cough through the day. The cough came sometimes in paroxysms. Has almost no expectoration; what does come is whitish. Has pain on coughing over sternum. Tongue clean in front, whitish at back. Appetite not very good. Bowels costive. Pulse quiet. Catamenia regular; last a week. Much dysmenorrhæal pain at time of menstruation. She is pale and delicate looking. Complains of being very easily tired. Is rather thinner than she used to be. No night sweats; sleeps well after the cough ceases. On examination of the chest, the right side is not so full as the

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left, but could hardly be said to be flattened. Nothing else abnormal. Nitric acid 1x, 2 drops three times a day.

She came back in ten days, feeling much better and stronger. The cough was almost gone, as also the chest pain. Bowels acting more regularly. Tongue cleaner. Appetite not yet good. Continue acid. She soon after this got quite free of the cough, and became quite well. I omitted to say that for the dysmenorrhœal pain I ordered *gelseminum* 1x, which relieved it very much.

Case IV.—M. B., æt. 15. A delicate looking girl. There was a history of a pulmonic attack the previous summer, which probably had been pneumonia of the left apex. When I saw her she was very thin and pale, and had for some weeks been getting thinner, and sweating at night. The appetite was poor; the tongue clean and rather red, and she had feeling of fulness in stomach after eating. The bowels were regular, and catamenia had been present several times. She was thirsty. Pulse quick; temperature high. She had a bad cough, with a good deal of muco-purulent expectoration. The cough came in paroxysms, was worst in the morning and at night. She had quick breathing, and shortness of breath on any exertion. On examination of the chest the left apex was found flattened; there was dulness on percussion on this side, with tubular breathing, prolonged expiration, increase of vocal resonance, and dry creaking sounds. I had her put to bed, treated with *aconite* and *ipecacuan*, and poultice to chest. Subsequently I changed the medicine to *arsenic*, and afterwards to *phosphorus*, under which treatment, by the end of a couple of months, she was immensely improved. The cough had ceased to come in paroxysms, and only in the morning was she troubled with it. Through the day it was only occasional, and at night there was none of it. The expectoration was now slight, and more mucous. She slept well, and was almost free of night sweats. The appetite was good, the pulse quiet, and the temperature normal. She had gained very greatly in flesh. The physical signs were improved. There was rather less dulness, but still the creaking sounds were present. I then put her on *nitric acid*. After a fortnight of this the cough was almost entirely gone, the expectoration also reduced to a minimum, the creaking sounds almost entirely gone.

I continued the acid for another fortnight, at the end of which she was going about, and so well that I left off seeing her.

Case V.—Miss R., æt. 13, had had for one or two winters during the coldest weather, and in spring, been with a cough. When I first treated her she had a dry, tickling cough, which began in the morning, continued all day, and on lying down at night, but left her after she fell asleep. She was in good

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general health in every way, and would have been quite well, were it not for the cough. There was no pain in the chest, nor soreness in the larynx on coughing, nor was there any tenderness on the larynx. On examination of the chest nothing unusual was found, except a slight prolongation of the expiration on the right side, and the expiratory sound very slightly blowing. Medicine after medicine, including the usual laryngeal ones, were tried, but with little effect, till at last she said she was tired of taking anything more. I at last thought of nitric acid. Under this the cough disappeared altogether in a week or ten days. Since then she has had a return of the same cough two or three times, when I at once gave the acid, with the same agreeable result.

Case VI.—A brother of the foregoing, a year and a half younger. He had a cough of precisely the same character. His general health was excellent, but this cough gave considerable uneasiness to his parents. His chest was perfectly sound, and there was no pain either there or in the larynx. Until I gave the acid to him the cough continued, in spite of other medicines, but under the use of the acid it disappeared in a few days. In his case also, I have had occasion once or twice since the first time to prescribe the acid for a return of the cough, which has each time disappeared very quickly.

These are the two cases which I referred to in the first part of this paper, and which I considered not as laryngeal coughs, but as due to a constitutional susceptibility of the bronchial mucous membrane to atmospheric influences.

Case VII.—Mrs. B., æt. 33, came to me complaining of attacks of shortness of breathing, which came on early in the morning with a cough, which came at that time in paroxysms, and which obliged her to sit up in bed. Very little expectoration was coughed up. Her breathing did not become free till after her breakfast, the cup of tea seeming to have the effect of relieving her. Through the rest of the day she had no trouble with her breathing, and no cough, and in fact was easy till the same time the next morning. This was not the first time she had been attacked in this way, but had been liable to it occasionally for years. She was in every other respect quite well, except that she was feeling rather weak, from having nursed her baby too long, and the bowels had become very costive. On examining the chest, the only abnormal signs present were an increased resonance of both fronts, with feeble breathing. There were no bronchitic sounds. I at first gave her arsenic, which for some time relieved her. It then lost its effect, and I prescribed nitric acid. Had this failed, or had there been bronchitic sounds, I should have given ipecacuan. When I next saw her, about a week after, she said the medicine had acted

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like a charm, and that for the last three or four mornings she had been perfectly free of cough and shortness of breathing. I then told her to stop the medicine, which she had begun to do that day of her own accord. I also must not forget to notice that the bowels had become quite regular and easy.

I could give a larger selection of cases, but I do not wish to weary my hearers; and I think the cases I have given are sufficient to illustrate the principal points I have taken up. I may be allowed, however, to add three cases, where the medicine was given specially to remove constipation.

Case VIII.—A lady, of nervous temperament, and subject to frequent pains of a rheumatic gouty character. Complained to me of very troublesome constipation. She was not generally troubled in this way, as the bowels as a rule acted regularly. The only other symptoms she had at this time were a great tendency to acidity and flatulence in the stomach, necessitating very careful diet. The tongue was clean. All the usual remedies for constipation were tried, but without success. Even an enema failed to produce a satisfactory motion. I then ordered nitric acid in doses of 2 drops of 1x, three times a day. This at once had the desired effect, and after taking it for the best part of a week, the bowels required no further assistance.

Case IX.—Mrs. D., æt. 69, came to me complaining of dyspepsia of long standing, the exact symptoms of which I need not waste time by enumerating. Along with this she had obstinate constipation, for which she had for a number of years taken purgative pills. Under treatment which it is unnecessary to enter on here, she completely got well of her dyspepsia, and the bowels improved, but not to such a degree as to give her comfort and satisfaction. I then prescribed nitric acid, 3 drops three times a day. After a week of this the bowels had begun to act quite easily and regularly, and she did not find it necessary to report herself again.

Case X.—Mrs. —— had been long confined to the house, and for a good part of this time to the sofa, on account of a tedious and chronic uterine affection. The bowels at first gave little trouble, being always relieved when necessary by an enema. After some time the constipation became such a troublesome complication, that it amounted to fœcal obstruction of the bowels. I had to give temporary relief by a full dose of castor oil, after which I tried every medicine I could think of as likely to be of service in preventing the same thing again happening. I was much disappointed at the failure of every one, the obstruction again recurring with much pain, while

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enemas of water, gruel, and oil came away as they were thrown up. In this state of matters, after having been obliged again to resort to the castor oil, I prescribed nitric acid; next day the bowels moved naturally, and continued to do so till I left off the acid, in order to return to the treatment specially directed to the uterine complaint. The constipation again returned, but the action of the acid, when all other homœopathic remedies had failed, was very striking.

I must apologise for the length of this paper, but as it would not have been complete without illustrative cases, I hope I may be pardoned.

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ON THE ACTION, SELECTION & ADMINISTRATION OF DRUGS.*

By R. D. Hale, M.D.

Mr. President and Gentlemen,—

Some apology or explanation seems necessary for bringing before the notice of the Congress a subject which, for its full consideration, would require at least an hour, but the subject having been dwelt upon at some length at the two last Congresses, the Congress being now assembled in London seemed a fitting opportunity to refer to the important communications of Drs. Madden and Sharp. Communications of the greatest possible importance, not only in relation to homœopathy, but to scientific medicine generally. For when we consider how rapidly knowledge is accumulating upon other subjects connected with the medical art, the discoveries in minute anatomy, micropathology, in experimental physiology, in electro-magnetic science in relation to physiology, especially as regards the functions of the various parts of the nervous system, in short in every department of medical knowledge, with one exception, namely, the discovery of a scientific basis upon which to found a rational system of therapeutics, it must be acknowledged that the questions raised by the essays of Drs. Drysdale, Sharp, and Madden, are questions which must sooner or later engage the attention of every thoughtful practitioner of homœopathy.

It may be said, perhaps, we homœopathists possess already a therapeutic basis; we have our law of *similia similibus curantur*, and far be it from me to undervalue that law which I have endeavoured myself to obey in practice loyally, but I ask myself, and I ask you, is it the *summum bonum* of our reformed system. Can a mere formula, however valuable, express all that nature has to reveal? do we not feel that there is some higher and deeper expression which is not only an empirical law, but an induction; not merely a formula, but a scientific fact? A firm conviction that it is especially our mission to carry out investigations to this end, assisted as we are by the labours of other explorers of another school,

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who although, alas! not of us, are working out problems of incalculable value for us, and may ere long be one with us, in recognising the existence of that higher law which, I believe, only requires time and patient working to bring to light. I do not presume to lay claim to any special fitness for bringing this subject before you, but I hope to be able to suggest some germs of thought which may fructify in your minds, and lead to practical issues in our onward course, as physicians less anxious for the maintenance of a dogma than for the establishment of a truth. Obstacles to the progress of our reformed system of medicine have been in the past mainly the opposition from outside, but to my mind the hindrances to the advance and recognition of our mode of practice are now more likely to exist within our own ranks, either by our older members stagnating into mechanical routine, or by our younger practitioners not only undervaluing the experience of Hahnemann's earlier disciples, but also in neglecting the individualisation of cases, the patient study of the materia medica, and the administration of those doses which the experience of the last fifty or sixty years has proved to be the best.

I entertain the hope that every advance made either in the sciences immediately connected with medicine, or in those collateral sciences bearing upon medicine, will enhance our admiration of our illustrious founder's originality of mind in discovering a law for the administration of drugs, which will I believe in time be found in harmony with other laws of nature. With these few introductory remarks I will now enter upon the subject more in detail, regretting, however, that the time allotted to me is quite insufficient for a more complete explanation of the views I have the honour to submit for your consideration.

If in the observations I shall lay before you I should appear to be employing the faculty of imagination in the treatment of my subject, I seek my justification in the words of Professor Tyndall, who writes thus: "How are these hidden things to be revealed? How, for example, are we to lay hold of the physical basis of light, since, like life itself, it lies entirely without the domain of the senses? Now philosophers may be right in affirming that we cannot transcend experience, but we can at all events carry it a long way from its origin. We can also mag-

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nify, diminish, qualify, and combine experiences, so as to render them fit for purposes entirely new. There are tories in science who regard imagination as a faculty rather to be avoided than employed. They observe its action in weak vessels, and are unduly impressed by its disasters. But they might, with equal justice, point to exploded boilers as an argument against the use of steam.

"Nourished by knowledge patiently won, bounded and conditioned by co-operant reason, imagination becomes the mightiest instrument of the physical discoverer. Newton's passage from a falling apple to a falling moon was, at the outset, a leap of the imagination. In Faraday the exercise of this faculty preceded all his experiments. In fact, without this power, our knowledge of nature would be a mere tabulation of co-existences and sequences. We should still believe in the succession of day and night, of summer and winter, but the soul of force would be dislodged from our universe, causal relations would disappear, and with them that science which is now binding the parts of nature to an organic whole."

Most of us will, I am sure, confess that a more satisfactory and rational principle in the application of our remedies is to be sought for. We ought not to be satisfied with a mere tabulation of symptoms, for which we must find, in a mechanical sort of way, the proper medicine. This method has no doubt helped us, and will continue to help us, until we discover a better one. But I would ask, does it satisfy any minds, but such as are content to work on in the beaten track of symptom covering? Will it satisfy the younger men, who come after us, nurtured in the allopathic schools, where knowledge is daily increasing in depth and in accuracy. Will our therapeutic law alone meet the demands of the medicine of the future, unless proved to be in harmony with other, and it may be, higher laws, which are being revealed to us? My object in this paper is, to press upon you the necessity of keeping pace with the scientific discoveries of the present day, and by moulding them to our therapeutics, raise the latter from an empirical formula to the higher standard of a scientific principle. Having the enormous advantage of a law of cure to start with, we can the better hold our vantage ground by utilising every discovery which throws light upon our

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special subjects of study. While deprecating a servile following of the Hahnemannian method of selecting the remedy, I must not be understood to undervalue symptoms apart from their pathological connection, for, as we all know, in a large number of cases we cannot accurately trace the co-relation. We succeed, without knowing why or how. We should wish and try to know. My contention is, that it is our duty to try. It should be our high aim and resolve to raise homœopathy from the mere formula, however true to a higher law of cure, based upon a more intimate knowledge of the essence of diseased action, and the relation which remedial agents hold to it therapeutically.

It will be necessary for a clear understanding of my argument to place a few thoughts before you in the form of axioms, thus—

Every organ is composed of cells containing germinal matter; cells are composed of molecules; molecules have motions or vibrations, and these vibrations or motions may be normal or abnormal. The healthy function of an organ depends upon the normal motions of the molecules composing its cells. Cell life depends upon suitable pabulum, and becomes abnormal, owing to an unsuitable or insufficient supply of pabulum, and also by agents which affect the arrangement or motions of the component molecules.

By the first causes healthy nutrition becomes diseased or depraved. The second factors, which we call toxic agents, disturb, arrest or destroy healthy molecular action.

To restore healthy action we employ agents of various kinds, but we have only now to do with those called drugs, which we say must be administered according to a law of similarity. The idea that these agents act as stimuli has been advanced by many, but it is a mere assumption I think, and likely to lead to error in practice, and had better be avoided. I suggest they act dynamically, in some cases, reversing, in others energising abnormal vibrations.

Two conditions appear to be necessary in order to secure the most perfect specific effect:—

1st. That the pathogenetic effect of a medicine upon a healthy subject should have the closest relation in *kind* to the abnormal state initiated by natural disease.

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2nd. The curative dose should hold the closest relation potentially to the normal rate of vibration which disease has perverted or disturbed.

We are now prepared to go a little deeper into this confessedly difficult investigation. By the light of increasing knowledge, it may ere long be possible to determine the exact relation which exists between remedial agents, the various organs for which they have an affinity, and the functions the various organs perform (Dr. Sharp's Essay on Organopathy has made a beginning in this direction). We know this much to start with, that all the organs of the body are energised by forces emanating from the ganglia of the sympathetic and the cerebro-spinal nervous systems; whether the tissues acted upon be muscles or arteries, or secreting glands, the work done and the functions performed take place in the minutest structure of each organ by cell growth, metamorphosis, and disintegration, and these again result from vibrations of the ultimate molecules of which each cell is composed, here are carried on the vital processes of health, and here also the changes and perturbations we call disease; and to these recesses must our curative agencies reach. The ganglia are affected by three kinds of excitants—chemical, mechanical, and electro-motor; it is with the last of these that drugs have especially to do. In speculating as to what is the *primum mobile* of a departure from health in any organ, the conjecture is not, I think, irrational, that withdrawal of electro-motor energy is the first link in the chain of causes; if partial it causes a temporary arrest of function, if more prolonged, organic change is the result; if complete or permanent, it is the death of the past. For example, *belladonna* in a pathogenetic dose at first quickens the heart's action by paralyzing the cardiac branch of the *par-vagum*, extending its influence to the nerve cells in the substance of the organ. *Nicotine* does the same, but its influence, it is proved, does not extend to the nerve cells, but only to the muscular fibrillæ, but if the dose be large enough in either case, there is a total arrest of action, and death follows.

The diminution of electro-motor force is followed in the case of secreting glands by similar results. First, functional deficiency, or a vitiated quality of the particular secretions. Cell metamorphosis becomes perverted, and either disorganisation of tissue or heterologous for-

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mations occur, if the nerve influence be more permanently withdrawn. During the progress of disease, complicated changes occur, involving other tissues and remote organs, a consideration of which would lead me too far into general pathology; what I have said only suggests the cause of the very earliest departure from health. But whether in the earliest or latest phases of pathological change, our cure work ought not only to be as strictly in conformity with our law as to the *kind* or quality of the drug, but also in the matter of dose, as regards its quantity, it ought to be in as close a relation as possible to the amount of dynamic force necessary in each case.

If then it be granted that vital actions in health and disease are carried on by vital processes in the minute structures of the body, and through the instrumentality of still minuter forms of matter, ought not the physical condition of the substances we employ as medicines to be in doses relatively minute, apart from any other quality they possess, either of elective affinity or dynamic force. But my contention is that *quo ad* minuteness there is dynamic power. Some most interesting provings of *belladonna, conium, calabar bean*, and *strychnia*, though not by the better homœopathic method, have yet elicited important facts, which may be turned to account by our school. It is found, for example, that *conium* and *belladonna* lessen the excitability of the nervous fibrillæ. *Conium*, acting specially upon the terminal fibrillæ of nerves in muscular fibre; *belladonna* acting more upon the central fibrillæ. *Calabar bean* and *strychnia*, on the other hand, increase the excitability of nerve tissue. The minuter shades of pathogenetic actions of these medicines it is the glory of homœopathy to have discovered, but without dwelling upon these, we may, I think, derive some assistance in endeavouring to understand something about the *modus operandi* of these toxic agents. All these drugs have, it seems, an affinity for nerve tissue, but the kind of actions they produce is not merely due to the affinity, but to the kind of molecular vibration which they set up. *Conium* and *belladonna* would appear to diminish, *calabar bean* and *strychnia* to increase the rates of vibration; however this may be, the two former are known to be antidotes to the two latter. Now, if natural disease has caused the diminished or increased excitability in nerve tissue, causing a disturbance which, in its essence,

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bears the closest resemblance to the effects of the toxic agent, that drug, whatever it is, is the true *similimum* to the abnormal condition. It may be objected, this is only another way, and a round-about one, of expressing *similia similibus*. My main object in these remarks is, to induce you to believe with me, that without discarding our convenient and venerable formula, we may hope for the time when we may find a more scientific expression, based upon a higher law, the outcome of extended knowledge, of a kind similar to what I have endeavoured to suggest to you. I am so convinced of the necessity for our future investigations, travelling in this direction, that looking to the future of homœopathy, I see no hopeful prospect for it, or indeed for any progress in medicine at all, if this field bearing precious fruit lies ignored and uncultivated.

A few words now on a subject bristling with difficulties —the question of doses. The chief difficulty of aiming at scientific accuracy depends upon the ever varying conditions of age, temperament of patient, nature of disease, upon its acuteness or chronicity, the properties of the drug, and other vital conditions which belong to living organisms.

The dose I suggest should be regulated by the nature of the tissue, and also by the rapidity or slowness of metamorphosis, still more by the greater or lesser excitability of the tissue, in relation to electro-motor force. For example, suppose *phosphorus* to be the remedy equally appropriate to disease of bone, and also to disease of brain, molecular vibrations and cell life are carried on with relative slowness in bone, and it has a low degree of excitability; a low dilution would seem to be proper for disease of bone tissue. In the nerve tissue of the brain we have exactly an opposite state of things. A medium or high dilution of phosphorus seems here most suitable.

This example I merely glance at to indicate the principle I wish to enforce, namely, that whenever it can be applied in practice, we have a more satisfactory guide than any formula like Dr. Drysdale's, which describes a true specific, "a remedy which cures by the absorption of its whole physiological into its therapeutic action." Accepting this as a truth, others have defined the curative dose to be that which falls short of its physiological action; but as the physiological action of any drug is not

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a fixed, but an ever varying quantity, such a rule is practically useless. Dr. Sharp throws a new light upon the subject; is it more helpful? He tells us he has by experiments on himself, arrived at the following induction, "that the law, *similia similibus curantur*, remains true when limited to large doses; and the law, *contraria contrariis curantur* is true when limited to the action of small doses." He thinks he has proved that the small dose reverses the action of the large, and he suggests the idea that the best antidote for a virulent poison, especially snake poison, may be very small doses of itself. These inductions are so startling, and so contrary to all preconceptions, that they require more numerous, and careful experiments to be made, free from the sources of fallacy to which Dr. Sharp's experiments are open, that we must, I fear, hesitate to accept them until proved more convincingly, however attractive and interesting. Did time permit, I think it could be shown that the phenomena Dr. Sharp adduces are to be explained by the recognised fact that drugs have primary and secondary actions.

The subject of dynamization I must discuss as shortly as possible. If minute subdivision, by multiplying the surfaces of contact between the drug and the organ, to be acted upon, is the only explanation of its dynamic power, then the greater the subdivision the greater ought to be the increment of power; but such is not found to be the case. I, therefore, contend that we need some other explanation, which I hold is this, that during the process of attenuation, the forces which before were potentially latent, become dynamically energetic, not because any new force or property is added, and certainly not owing to the absurd notion that the spirit of the drug is liberated from matter, but that molecular changes occur, analogous to those which take place in other kinds of matter, the result of recognised physical laws. All the arguments against the theory of dynamization have been chiefly and properly directed against the metaphysical theory, but none that I have read attempt to refute the facts which the physicists furnish us with, and it is with these I submit that the theory is in harmony, and by these, reasoning from analogy, the doctrine of dynamization can be proved true. A bar of steel is rendered magnetic by certain mechanical vibrations of its molecules; it

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can be demagnetized by other kinds of vibrations. By alloying mercury with a *millionth* of its weight of sodium, a force of not less than 50,000 times that of gravity is generated by a galvanic current. The difference between albumen and animal fibrine is less owing to diversity of composition than to a change in the arrangement of the ultimate particles. I contend, therefore, that there is no reason for supposing that drugs form an exception to other kinds of matter, or that they are not governed by the same laws. Such considerations as these, I think, go a great way in establishing the reasonableness of the theory of dynamization, require no idea of spirit or essence apart from matter, may be accepted equally by the vitalist or the materialist, do no violence to scientific inductions, but do afford a satisfactory explanation of the action of fractional and infinitesimal doses, when brought into contact with diseased conditions of the living organism, and I hazard the conjecture that different potencies of medicines are more curative than others, in proportion as their rates of vibration are, so to speak, in harmony with the rates of vibration in the diseased organ.

In conclusion, I beg to thank you for the patient hearing you have given to these necessarily hurried remarks, which I hope you will value for just what they are worth, and discuss them if, in your opinion, they are worth discussing.

NOTE.—Since this paper was read the following remarks appear in an American contemporary:—"Morbid forces vibrate in a certain organ or tissue of the body; an atomized drug whose forces are similar, and whose similarity has been already proved on healthy persons, begets a series of similar waves in the part diseased. Of independent origin, they continually interfere with the waves of disease-force in the cells of the tissue, in the nerves of the organ, and in the whole sympathising body. Where disease was active, now come the lines of rest—there is comfort, where a little while ago was excruciating pain; there is quiet sleep in lieu of wakefulness and tossing; there is a sane brain instead of delirium; a soft pulse in place of the fevered circulation; the diseased action has been cured, neutralised by the similar, the homœopathic action of the specific counterpart of that diseased action, embodied in the drug; and we have furnished a new demonstration of the well proved law of cure—'likes are cured by likes.'

"Indeed, so absolute is the demonstration of this principle in all departments of dynamics, so well understood is it by all scientists, that to deny it in medicine, to decry, ignore, or neglect a system of cure, a science of therapeutics based upon it; or, worse than this, to persecute or denounce such a system, seems like sheer infatuation."

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ON MALIGNANT GROWTHS.

By Edward T. Blake, M.D.

Mr. President and Gentlemen,—For addressing you this day I have selected a subject that commands our attention even for other reasons than that it will ever possess for suffering humanity an interest as profound as it is painful.

The various names connected with this subject are used in so very vague a way, that I must begin by a definition of terms. For the excellent reason that there is no absolute anatomical definition, I shall use the term "malignant" in a purely clinical sense, not as interchangeable with cancer, but as including those morbid products, lupus, sarcoma, myxoma, recurrent fibroid, singularly inflexible to treatment, prone to recur, and ready, under certain peculiar conditions of the constitution, to take on action markedly malignant in the fullest sense of that term.

By "cancer" or "carcinoma" I shall always mean the actual malignant product, and, following the analogy of tubercle, I shall call the cancerous diathesis or tendency to carcinoma "carcinosis;" the diseased condition itself "carcinisation," by that I mean the actual developed presence of the malignant growth.

The nomenclature of malignant disease is indeed, like that of disorders of the skin, capable of advantageous simplification. Such names as "colloid" or "gelatinous," "fasciculate," "cystic" and "alveolar," denoting merely adventitious conditions frequently developed in one part only of any foreign growth, should be discarded, or at the least only retained for such special uses as minute clinical description. The term "osteoid" should not be used for tumours, which commence in bony tissues, (though osteoid tumours do usually arise in bone,) but for tumours which show a tendency to general ossification, and that early in their history.

2ndly. Names involving a theoretic supposition as to contents, *e.g.*, "atheroma," "melleceris," and "steatoma," are objectionable.

3rdly. The amount of blood or of pigment is too acci-

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dental and fluctuating to be made a criterion of differentiation; therefore, such words as "cerebriform," "hæmatoid," "melanotic," "fungus hæmatodes," "villous," "medullary sarcoma," should only be employed to qualify a form or express the condition of a cancer not as primary designations.

The basis of classification of cancerous tumours must, for reasons that we shall presently see, be always clinical. When, thanks to the microscope, it was shown that there is a law of malignancy, namely, that *the more widely divergent from the normal cells of the part, the greater the degree of malignancy of a growth*, high hopes were raised that now a morphological and strictly accurate method of arrangement might be obtained. Alas! it was found that these rebellious disorders would not submit to the rules imposed upon them.

Some highly heterologous tumours, *e.g.*, certain cases of scirrhus, were characterised by slow growth and tendency to self-cure, whilst instances of eminently homologous growth, as recurrent fibroid, myxoma, lymphadenoid sarcoma, enchondroma, and other histioid growths, exhibited a far higher degree of malignancy. It is now known that malignancy bears a distinct proportion to the mobility of elements: instance, infantile ocular glioma: and is greatly influenced by such extraneous conditions as position, involving exposure to foreign agencies; and such internal conditions as warmth, moisture, vascularity, free lymphatic connections, occasional exalted functional activity, &c., &c.

A physiological basis of classification is too simple, for by it we could reduce all cancers to one kind, showing that the soft forms are produced merely by predominance of the cellular element; the hard, by excess of fibrous stroma; osteoid by ossification of the stroma; colloid and cystic by liquefaction of the cellular interspaces, and so on.

Histology would have the opposite drawback of being too elaborate. The variety of cells is so infinite that we should have a classification far too complex for clinical purposes.

It is striking and suggestive that physicians who have attempted pathological classification have always introduced the clinical element. Rokitansky, in 1855, gave six kinds of cancer; Paget, in 1853, was not content with

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less than eight sub-divisions; the College of Physicians, in 1869, gave five kinds; whilst Walshe and Bennett rightly conclude all kinds of cancer under three groups.

Scirrhus,
Encephaloma, and
Colloid;

and if for the word colloid* we substitute cancroid, to include epithelioma, lupus, and similar growths, we have a grouping perfect, because simple, and yet sufficiently comprehensive for all clinical purposes.

I have spoken of following the example of Sir William Jenner's nomenclature of tubercle, and, though analogy be both safe and serviceable in matters of terminology, it is by no means stable ground in the region of pathology.

Yet, for the minds of men, analogy possesses a singular fascination. An interesting instance is the influence, perhaps unconscious, which the analogy between tuberculosis and carcinosis, has evidently exerted on the whole complexion of medical opinion on this subject. More especially has this been the case since the experiments initiated by Cruveilhier† and Alison,‡ and more recently prosecuted by Marcet, Villemin, Wilson Fox and Burdon Sanderson, have distinctly demonstrated that tubercle, far from being a specific morbid growth, is an adventitious product scarcely organic, and capable of being produced under all kinds of differing circumstances and from many diverse materials.

Men have begun to ask—Might not the "cancer cell" be, like the "granulations of Bayle" (as tubercles were styled after their discoverer), a local product modified merely in metamorphosis.

To this view the cellular pathology of Virchow gave a marked and important impetus.

As a result naturally to be looked for, a vigorous young school has sprung up whom we may style "the localists."

* I dismiss "colloid" as a main group, because, though a few tumours may be primarily colloid, they nearly invariably occur as a form of degeneration of other cancers.

† Cruveilhier introduced tubercles from the human subject into the thigh of a dog, and found that their presence gave rise to distinct tubercles.

‡ Alison, of Edinburgh, injected mercury into the thorax of a dog; then, without knowing what had been done, Abercrombie and others, on carefully examining the lungs, pronounced them tuberculous.

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Their essential article of faith is, that until after gland-invasion (the cancer-cell having then been precipitated into the general circulation), carcinoma is emphatically a local disease.

They point triumphantly to the frequent history of local lesion—the dirty scrotum of the sweep, the worry of old ulcers, the fretted scars of ancient burns, the marital blow on the bosom, the corroding cutty-pipe, the irritation of the solitary fang as opposed to the dubious "cancerous diathesis," and the still more apocryphal "hereditary history"—"the twin bug-bears," say they, "that have so long haunted the mysterious and gloomy shades of archaic pseudo-pathology."

And indeed, as to the former of these two pillars of "the constitutionalists," we have all encountered cases where no cachexia made its appearance (*vide* Case 1); and as to the latter, it is rather striking that two of the most respectable of "the generalists," Velpean and Sir James Paget," * from personal experience, arrive at the same result, viz., that the non-hereditary cases are certainly twice as numerous as the hereditary.

But one of the most striking observations bearing on this point is that of Moxon, that a remarkable morphological resemblance exists between the constitution of the secondary deposits and of the primary growths, however distant in site and varied in seat. Thus, in hepatic deposits, secondary to rectal cancer, bodies are found resembling the follicles of Lieberkühn, and ossification is seen in those cancerous deposits of the lung which are secondary to osteoid chondroma of the bones of the extremities.

Nor is the local nature of cancer by any means a merely theoretic question, because on it the entire consideration of operation hinges. If cancer be—to use the well-known phrase—"in the blood," it is plain that to operate is only to expose the patient to the horrors of the knife and the depressing influences of hæmorrhage without commensurate gain. But is cancer in the blood?

* Paget's experience, recorded in 1857, was that whilst there appears to be a tendency to the hereditary transmission both of cancerous and non-cancerous growths, there is a much greater probability of the hereditary transmission of cancerous than of non-cancerous growths, and that in the proportion probably of 22·4 for the cancerous and 6·2 for the non-cancerous.

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If we compare carcinoma with other disorders certainly laying more claim to the title blood disease, we encounter contrasts quite as striking as the points of resemblance. Take, for example, syphilis, certainly inoculable; whereas all efforts hitherto made to induce carcinisation by inoculation have failed. Dupuytren introduced cancerous growths into the stomachs of different animals—and injected the discharges into their veins and into their various serous cavities without result. Alibert repeated these experiments, and even had the temerity to inoculate himself with cancerous secretion, yet no peculiar pathological phenomenon followed this bold experiment. More recently Harley and Lawrence have experimented upon dogs, and have arrived at the same conclusion. I have myself encountered many instances of women having the uterine neck nearly destroyed by carcinoma, but I have never seen it communicated to the husband; and again, it is well known that. even under these circumstances children may be born who enjoy a life-long immunity from any cancerous manifestation.

It would seem to be a work of supererogation to attempt to demonstrate, in the present day, that carcinoma is not a blood disease in the sense of residing in the blood. I will content myself with suggesting five points for your consideration—

1. No peculiar condition of that fluid has ever been demonstrated.

2. If a blood disease, why is it primarily local? It should be as diffused as the blood itself.

3. It is highly improbable that a tendency so persistent should locate itself in the most fugitive and ever-changing portion of the body. It is only necessary to remember that the blood is driven into every corner of the body 100,000 times a day; that it is perpetually filtering through the tissues; that it is undergoing a constant conversion into the substance or secretion of the body, to be convinced of the utter untenability of such a view. At the bedside we see that when any morbid material is introduced by art or accident into the circulation, nature does not hide it and store it up for half a century, but proceeds straightway to expel it by the excretory glands. If cancer be a blood disease, we should find it most prevalent in infancy, and it would attack by preference the emunctory organs—the intestinal glands, the lungs, the kidney, and the skin. This we know is not the case.

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4. If cancer be "in the blood," why does it return at the spot operated on, and not, say, in the opposite breast, for it has a far stronger elective affinity for the mamma than even for a cicatrix?

5. Cancer is often transmitted directly from father to offspring. This cannot be through the blood, but by the spermatic fluid.

Arising, doubtless, from Lebert's unfortunate phrase, a great deal of rubbish has been talked about the "specific cancer-cell." Of a "typical," or a "characteristic" cancer-cell, we may speak, but of a "specific cancer-cell" we may not speak; and for this reason, that a "specific cancer-cell" does not exist. Nor can the importance of remembering this be too strongly insisted on.

I need not remind you that every kind of cell in every kind of growth ever called malignant, finds its true morphological analogue in some element of normal tissue. Nor, when we call to mind what an elemental character cancer-tissue presents—what a rich vitality appears, co-existent with so very low a type in the physiological scale, need it surprise us to find that it is in the embryonic period of life that we must search for the prototype of the cancer-cell.

One of the most remarkable results of modern microscopic research is the discovery of the morph logical relation between the characteristic cell of carcinoma and the cells of the blastodermic membrane of the embryo.

Thus we see that we cannot isolate a cell and, pointing to it, say—"This is a representative cancer cell," for cancer is a clinical condition rather than an anatomical entity. Even histologically, the arrangement of the cells goes for more than the actual form, and it is the history, development, growth, and degeneration that severally go to make up that *tout ensemble* which we style "cancer."

Cancer is certainly a disease of old age, as well as of abolished function. Undoubtedly it attacks women at a younger age than men (and therefore more women survive long enough to become cancerous), because the organs of reproduction fall earlier into abeyance; yet if this were the only reason, we should find cancer more frequently than we do in the thymus, in which gland it is, as you are aware, nearly unknown.

Although in the earlier stages cancerous growths often display what seems like a rich exuberance of vitality in

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the rapid proliferation of cells, still a little reflection convinces us that it is really a disease of debility.

Like the accumulations of vast numbers of epitheliel cells on a "loaded tongue," or of pus-cells in an unhealthy sore, cancer-cells are really arrests of development.

And, unlike a benign tumour, a cancer never establishes a permanent existence: from the first it tends to degeneracy and decay.

When the powers of life are at ebb-tide, and the processes of nutrition languid, we often see the liquefactive degeneration delayed, but it is only postponed for a time; there ever resides innate an irresistible impetus to ultimate liquefaction and absorption.

Walshe has pointed out that the mortality from cancer goes on steadily augmenting with each successive decade until the 80th year.

Just as the mistletoe flourishes best on the old and exhausted oak, so this truly terrible disorder marks for its victim the feeble female in preference to the robust male—the aged and decrepit rather than the young and vigorous.

Here I am reminded of a curious fact that must have forced itself upon us all—a fact that might be employed by the localists, viz., that though cancer selects as its prey those whom we are in the habit of considering the weaker section of the community, still it usually arises in persons enjoying, *at the time*, good health, and, excepting as a mode of degeneration of primarily benign tumours, it rarely figures as a sequela to other disorders. How marked the contrast presented in this respect by cancer with tubercle!

Though we have ample ground for considering cancer as a senile disease, yet the influence of age, as of sex, is apt to be over-estimated.

Taking a numerical standard so high as to be free from ordinary disturbing influences, we find that in the Registrar-General's Returns since 1838, there have been recorded 60·196 cases of carcinoma. Of these

18·059 were males,
42·137 were females,

a proportion of 7 women to every 3 men—not much more than twice as numerous.

Again, with regard to age, of the 60,000 odd cases, nearly 28,000 are under 45, and a vast number under 30.

Though many cases are not now termed cancer which

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were pronounced "malignant" by our fathers, the proportion of cases is enormously on the increase.

Carcinoma is a disease of civilisation—whilst it is quite common in our household pets and domesticated animals, in the wild races it is unknown.

Perhaps one of the most remarkable facts brought to light by the enforced Government Returns of Causes of Death is, that cancer possesses a distinct geographical distribution. Dr. Haviland has, with painstaking patience, produced no less a wonder than a "Cancer Chart," showing the districts where cancer is endemic. One of the singular results of this inquiry is to show that cancer is rare on the primitive rocks and on the early formations; whereas in the river courses, in the tertiary formations, and especially in low-lying alluvial lands, prone to floods, carcinomatous disease is much more prevalent.

The question of the local or constitutional character necessarily obtrudes itself on the consideration of prognosis in cases of cancer. The views of the localists and of the generalists are without doubt, at first sight, decidedly opposed; but there is, as we usually see, a measure of truth on both sides. The way of safety is "*la juste milieu.*"

Cancer is, in its own nature, as regards the primary growth, emphatically local, as evidenced by its singularity—the primary growth is nearly invariably unique. It may be as local as the cheesy deposit resulting from a catarrhal or a traumatic pneumonia, and just as, if the pneumonic subject be free from phthisical tendency the tubercle will become obsolescent, so, if there be no cancerous history, the cancer will possibly die a natural death by degeneration. If carefully removed, carcinosis not being present, it may not recur; but let a cancerous ancestry have impressed on every cell in the body the tendency to malignancy, and woe betide the unfortunate sufferer—his cells run riot, cancer-mad! Just as our gifted *confrère*, who so ably represents our system at Bournemouth, clearly set forth when we met in Congress in 1872, that tubercle, occurring in the phthisical subject, calls for a grave prognosis, whereas in the tubercle of the non-strumous we are dealing with a widely different condition—a condition in which we may conscientiously hold forth high hope of recovery—so, though not to the same extent, with cancer. Growths undoubtedly carcinoma-

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tous in character have been removed, and have never, during a long succeeding life, recurred. Had but one well-authenticated case of this kind been recorded, it would serve to prove irrefragably that cancer may be a local disease, but it would not demonstrate that carcinoma may not be constitutional.

An obvious corollary to the preceding proposition is that, if a case of cancer present itself, in an early stage, in a young patient, free from cachexia and from hereditary taint, it is plainly our duty to advocate ablation.

As we have witnessed that the nomenclature of cancer cannot be based on the characters of the morbid growth, so now we see that for the formation of an accurate prognosis other than anatomical elements must be estimated. And just as in pulmonary phthisis none of us would dream of giving an opinion, gloomy in proportion to local lesion, but rather in relation to the marked presence or otherwise of the strumous diathesis, and having regard to the general powers of life; so in cancer, if there be hereditary history or not, if local conditions and general symptoms be favourable or the reverse, we give a cheerful or a grave prognosis.

With regard to diagnosis an interesting question arises. Can an absolute verdict be pronounced without the exploring trochar? I think that in the soft varieties of malignant disease an approximate estimate only can be arrived at. To illustrate this position let me briefly narrate the history of two cases. In one of these encephaloma was called abscess, in the other an ordinary lymphatic abscess was considered to be probably malignant in character.

Case I.—Was a young lady, aged *22*, who had not long "assumed the black veil." She came under my care with an enlargement of one mamma; the breast had only been painful for a few weeks. On examination I found a marked augmentation in size, but the contour of the organ was retained; the sense of fluctuation was perfect. The general health was good, the secretions normal, the skin of the body clear, the cheeks tinted with the hue of health, the expression of the face cheerful. The family history was vague. There was no evidence of irritation or of local injury. The leading allopathic authority of the place had pronounced the case non-malignant in nature. One of the best surgeons in our ranks saw the

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patient, and, diagnosing abscess, plunged in a bistoury; a few drops of blood only followed the withdrawal of the knife. Ulceration immediately set in at the point of puncture; the skin became extensively infiltrated; the lung was soon involved, apparently from anatomical extension rather than from secondary deposit, and in a few weeks her sufferings were at an end. This nun, to the last, retained her clear and healthy skin, plump figure, and look of health. Here was cancer without a trace of cachexia.

Then, to illustrate the converse—how closely abscess will simulate cancer.

Case II.—An unmarried lady of uncertain age, apparently about 70, with furrowed, wrinkled, and yellow skin, gaunt figure and blue lips, I attended in conjunction with an accomplished physician belonging to our body. This patient had a tumour in the anterior cervical triangle on the right side; the skin over it tense and red, extremely tender on pressure. There was hoarseness present from laryngeal ulceration. My colleague inclined to a malignant diagnosis, and if patient ever had cachexia well pronounced that patient had. I supported his view, gave *conium* 3x, internally, and compressed the tumour with *succus conii*. Under this treatment it broke, discharged copiously filthy and most offensive pus, and resolved itself into a chronic low-typed ulcer. The patient afterwards died with double pneumonia. I found a good-sized polypus, pendent from the os tincæ.

The physiological results of this polypus threw some light on the cachectic appearance. An exploring trochar would have spared both these blunders.

The employment of the trochar or of the aspirator certainly tends to expedite the ulceration of the cancer, and thus has a material effect in shortening life.*

As physicians our duty is to prolong existence under all circumstances, even when that existence is fraught with acute suffering. It becomes a point of casuistry whether we are justified in the use of this means of diagnosis? I judge so, because, 1st, it is often impossible to institute appropriate treatment without; 2ndly, the probabilities are so highly in favour of abscess, in which case

* A careful perusal of Mr. Oliver Pemberton's collection of cancer cases must convince any one of the truth of this.

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the trochar would do positive good; 3rdly, if the tumour be malignant, the chance of operation remains, and is then readily decided.

Treatment—Medical, Surgical, and Hygienic.

We pass now, gentlemen, to the consideration of the treatment of cancer. Whilst it is certainly my opinion that the specific remedy to either of the classic forms of carcinoma has yet to be discovered, I as implicitly believe in the curability of cancer. Every pathological discovery points indubitably in the direction that there are fewer and yet fewer essential differences between benign and malignant growths.

It is not my purpose to pass in review every remedy that has ever been suggested or employed even by our own school. Your time will be better occupied if we glance together at the remedies which have been followed by the greatest amount of success, and try to relegate each to its appropriate sphere.

With a general survey of our literature I will incorporate my own experience.

And first taking SCIRRHUS.

In the early stage the most specific remedy we possess is undoubtedly *conium*. All that Hahnemann can advance theoretically in its favour is found in his proving, "Pain in the mamma; hardness of her right mamma, with pain to the touch; and stitches in the mammæ at night." But with his usual sagacity the great Master saw that in a remedy so indicated in the results of local injury—so closely allied in its affinities to the lymphatic system—so often suggested by the characteristics of senile disease—we must have one very nearly related to scirrhus.

Over the second stage, or stage of degenerative softening, *hydrastis* probably exercises the greatest amount of influence.

In the third or ulcerative stage I have seen the external use of *galium aparine* followed by remarkable results, as the following instance will testify:—

Scirrhus.

Mrs. S. W., æt. 52.

July 10th, 1871.—Nine years ago she perceived a lump in her left breast. This gradually enlarged, and when she consulted me it had reached the size of an orange.

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The glands were much implicated. She was tortured with sharp and lancinating pains, but there is no burning. There is a small cancerous nodule above the scapular spine on the right side. There is cough, with yellow expectoration, especially in the morning; vertigo, backache, flatulence. The period appears intermittently; legs feel bruised; she is a weakly, thin, cachectic-looking person.

Under *conium* 3x, with *succus conii* locally, the symptoms improved, the tumour decidedly diminished, the skin became less engaged, and the general powers of life were markedly invigorated down to the 23rd of October—a space of more than four months. Then active disintegration took place, the tumour discharged at successive points, and various remedies were employed to meet new symptoms as they appeared.

On the 12th Feb., 1872, a nodule appeared in the right breast; *hydrastis* 3x, was administered internally, and an infusion of *hydrastis* applied to the tumours.

On the 25th of March a nodule appeared on the forehead. All through the course of the disease copious hæmorrhages had taken place from the ulcerating left breast.

Belladonna compresses afforded great relief to the pain.

At this stage I applied to the open sore, now as large as the palm of my hand, a compress of *succus galii*. The effect was very peculiar and striking. In a few days the wound dried, and simultaneously a diminution in the size of the secondary nodules was visible. The ulcer of the left breast became entirely desiccated and scarred over, whilst owing to the arrest in the discharge and hæmorrhage the general health visibly improved.

This patient took cold in August; acute pleurisy, with effusion over the right lung, combined with an aphthous state of the mouth, was the immediate cause of death. She expired on the 14th of August, 1872.

I have injected *succus galii* into scirrhus nodules in the first stage with purely negative results, and I may say the same of *hydrastis*.

Arsenic I administer when there is burning pain and great prostration.

Silica gives very marked relief to the secondary sympathetic pains of cancer.

Condurango was so highly lauded when it appeared that I have never employed it.

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Encephaloid.

There is no doubt that this form of cancer has disappeared under the use of specifically-indicated remedies. The celebrated case of Marshal Radetzky is sufficient to prove this. Unfortunately two remedies—*carbo animalis* and *thuja*—were employed. Until it has been demonstrated which of those actually should be credited with the cure, we cannot do better than repeat Dr. Hartung's prescription.

I think Dr. Hughes' observations conclusively point to *phosphorus* as the most hopeful remedy against the highly vascular varieties of encephaloma.

Epithelioma.

It is here that *hydrastis* finds its true sphere of action. On reading carefully the provings of the Golden Seal we cannot fail to be impressed with the peculiar and profound influence exerted by this drug over the mucous membranes of the body. I have so many times witnessed the rapid disappearance of epithelial lip-cancer under the combined external and internal use of this remedy, that I must claim it as an undoubted exception to my assertion that we have not discovered the specific to any classic form of cancer.

Should it in any special case fail, we should fall back on the use of the time-honoured *arsenic.*

Cancroid Growths.

Of these the representative is *lupus.* Its most appropriate treatment I have indicated elsewhere. Undoubtedly the most strongly indicated remedy theoretically, and the most successful in practice, is the *bichromate of potash.**

Surgical Treatment.

Sir Benjamin Brodie, the king of conservative surgeons, admirably defined surgery as "bad medicine." Our daily experience has amply confirmed his dictum. Surgical treatment is obviously of limited application in

* I should like it to be clearly understood that I do not undervalue symptomatic treatment. Upon that we must all fall back at times, to give those entrusted to our care the fullest benefit of the resources at our command. Here my object is different; I purposely approach the subject from a pathological point of view.

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malignant disease. It is plain that we can remove only superficial growths by external means.

The experience of physicians from the time of Hippocrates confirms the view that the use of the knife is a delusion, either as a means of cure or even of prolonging life. It is unfortunate that the statistics of operation mean the statistics of the knife. If a cancer evidently resist internal treatment, the best chance we can give our patient is by enucleation by means of caustics. The knife I never employ, for however clean the sweep, however careful the sponging, arteries, veins, capillaries, and lymphatics are left more or less patulous, and who shall say that no drop of cancer juice shall reach their gaping orifices? The most convenient caustic is undoubtedly the chloride of zinc. It is usually combined with hydrastis powder to form a paste, but I am convinced that the nature of the substance employed for that purpose is a matter of perfect indifference.* It may be mixed with flour, as was the custom with M. Cancoin, who, in conjunction with M. Hänck, is said to have first employed it as an escharotic.

My custom is to chloroform the patient, make a ring of carbonate of soda paste round the tumour *two inches* from its circumference. Armed with stout gauntlets I then destroy the area of skin between the tumour and the alkaline paste by painting with anhydrous nitric acid, using a glass brush. Care must be taken to thoroughly protect the circumjacent tissues from the spurting of the acid.

With a wire ecraseur I then remove the tumour, secure bleeding vessels by ligature or acupressure, and cover the exposed surface with caustic paste spread on lint, covered with oiled silk. Upon this may be placed a bladder of ice to allay pain.

The next day, if the base of the tumour be not reached by the paste, I make parallel incisions with a blunt knife, and apply paste again. The use of the ecraseur spares the tedium and torture of the customary repeated incisions. If the cancer be colossal the ecraseur wire may be passed through it by means of a blunt needle, and the tumour removed in two or more segments.

When the base of the tumour is reached, and, if subja-

* A very little reflection will convince any one that a compound intended to react destructively or chemically upon tissues, cannot at the same time exert a therapeutic action on them.

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cent tissues allow it, passed, I dress the resultant wound with *inf. hydrastis.* If healing be tedious, I apply *extract. galii ap.*, using as far as possible the same medicine internally as locally.

I think it judicious to exercise great care in the selection of cases for operation. I do not commend removal if the patient be fat and plethoric, or if the cancer cover an enormous surface; if the patient be of gross habit and overfed on animal food; if of drunken habit; if gouty; if the subject of albuminuria; if markedly dejected or cachectic; if skin be brawny, with firm œdema, and wide open hair follicles; if the skin be widely adherent or contain scattered cancerous foci; if the patient be aged, with little prospect of prolonged life in any case; if the glands be seriously complicated; if other organic disease be present.

As regards the hygienic conditions of cancer patients, after what we have said of the tendency of damp, low-lying soils to assist in the generation of cancer cachexia, it is obvious that a removal from them to high-lying primary strata, such as you are aware may be found in this country in the upper grounds of Scotland, of Wales, and of Devon.

I have seen no benefit accrue from depriving patients of animal food.

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ON THE PHYSIOLOGICAL AND THERAPEUTIC ACTION OF ALOES.

By W. B. A. Scott, M.D.

The date of the introduction of *aloes* into the medical armarium lies at some unknown point in the long interval between Hippocrates and Dioscorides; by the former the drug is not even mentioned, while the latter refers to it as in extensive use as a polychrest in his own days. More fortunate than many other remedies of, perhaps, equal value, *aloes* have, up to the present day, consistently sustained the high reputation they thus early acquired. They were held in high esteem by the Arabian school; and entered into a large proportion of the favourite nostrums of the middle ages. In the latest edition of the British Pharmacopœia they form an ingredient in about twenty officinal preparations, many of which, from the polypharmacy displayed in their composition, are evidently of no recent date. Very various are the virtues which have been ascribed to this drug at different times. Thus in *Hufeland's Journal* (1822) we find a case of deafness alleged to have been cured by *aloes*, and this action has been confirmed by other observers. In 1824 we find the drug recommended by Rau for dysentery, without, perhaps, sufficient pains having been taken to indicate the forms of that disease to which it is specially applicable. Again, in 1839 Biermann employed it externally in the treatment of burns. Among the allopaths of the present day, as is well known, it ranks high as a tonic, stomachic, purgative, emmenagogue, and occasionally as an anthelmintic.

Socotrine and Barbadoes *aloes*, the two officinal forms of the drug, so named from their respective sources, consist of the inspissated juice of the leaves probably of several different varieties, as *aloes spicata, vulgaris, socotrina, rubescens*, &c. That in general use among homœopathists is prepared from *a. spicata;* but the chief provings

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which I have been able to obtain (viz., those in the *American Homœopathic Review*, to which your Vice-President kindly drew my attention), seem to have been made with the ordinary Socotrine *aloes*. I have found it impossible to obtain other provings of any length, for which reason, among many others, I trust I shall meet with your kind indulgence for the very meagre and imperfect nature of the following remarks.

Under the head of "morale" we find the following leading symptoms ascribed to *aloes*:—Anxiety, anguish, restlessness, fear of death, repugnance to conversation, anthropophobia, disinclination to labour, amorous thoughts, despondency and moroseness. Accordingly, we find this drug recommended as a "divine remedy for melancholy" by Aretæus. All these symptoms seem usually to be aggravated during the morning and afternoon, and to be diminished towards night. They are also relieved in the open air. Symptoms of a totally opposite nature have been observed in some cases, but those above mentioned seem to be of the more frequent occurrence. It will be at once seen that they are all but identical with those of *aconite*, with the exception that the latter presents a nocturnal aggravation. Also, dislike to noise is specially recorded under *aconite*, and not so distinctly referred to under *aloes*, though even here *one* prover mentions a liability to start at sudden sounds under certain circumstances. The symptoms of *agaricus* under this head appear scarcely distinguishable from those of *aloes*; and here the period of exacerbation nearly corresponds. This is also true of *aurum*. The same may be said of *belladonna*, only here the aggravation is towards evening. *Colocynth*, *ipecacuanha*, *conium*, and *bryonia*, too, present striking resemblances, as also does *nux vomica*, though in the last-named drug the action is more variable. Something similar is perceived with *pulsatilla*. *Phosphorus* and *lycopodium*; also offers many points of similitude; while *platina*, *plumbum*, *assafœtida*, *arsenicum*, and *clematis* exhibit a partial, though less complete, resemblance.

For Morale; compare with *aconite*, *agaricus*, *aurum*, *belladonna*, *colocynth*, *ipecacuanha*, *conium*, *bryonia*, *nux vomica*, *pulsatilla*, *lycopodium*, *platina*, *plumbum*, *arsenicum*, *assafœtida*, *clematis*, *phosphorus*.

We find the following cerebral symptoms produced by *aloes*: Vertigo, worse on turning quickly, or going up-

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stairs; dull, pressive pain in forehead and vertex; hemicrania; stitch-like pains in supra-orbital region; pressive and boring pain in temples; pressive pain at vertex; pulsating pain at occiput; feeling of pressing asunder at occiput; congestion of the head; throbbing in the middle of the brain. Accordingly, we find *aloes* recommended for headache by Dioscorides; and specially prescribed in severe and prolonged cases of headache, accompanied with vertigo, by Schrœder and Trousseau. The head symptoms are relieved by cold, thus manifesting their congestive nature. We also find numbness and heat of the scalp, with dryness of the hair; and this drug has been recommended by Schrœder and others to prevent loss of hair. Again, we find pressive pain in the eyes with conjunctivitis, and find it given by Dioscorides for suppuration from the eyes. We find ear-ache, increased by clenching the teeth, tinnitus, and other aural symptoms. Its efficacy in some cases of deafness is well known. Lastly, we have drawing toothache.

For cerebral symptoms compare with *aconite, lycopodium, colocynth, pulsatilla, nux vomica, conium, bryonia, plumbum, belladonna, arsenicum, platinum, aurum, assafœtida, clematis, agaricus, graphites, phosphorus.*

Now, every one of these symptoms occurs with *aconite* and *phosphorus; agaricus* presents fewer points of resemblance than under the previous head; *assafœtida* still fewer; *arsenicum* a good many more; *aurum* and *platinum* have much in common with them, especially the eye-symptoms as regards the former, and the pressive nature of the headache in the case of the latter; the cerebral symptoms of *belladonna*, though far more marked, are not dissimilar in kind; those of *nux vomica, conium*, and *bryonia* approximate still more closely; but in the last case the symptoms seem more of a nervous than a congestive character; *plumbum* has much in common, and, what is very striking, produces frequent and sudden attacks of deafness; *pulsatilla* offers a very strong resemblance; *graphites* less so; *clematis* rather more than *graphites;* while the likeness presented by *lycopodium* and *colocynth* almost equals that of *aconite*. *Ipecacuanha*, the morale of which is a good deal similar to that of *aloes*, here presents few points of resemblance. The order of likeness seems to me as follows: *Aconite, lycopodium, phosphorus, colocynth, pulsatilla, nux vomica, conium, bryonia, plum-*

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bum, belladonna, arsenicum, platinum, aurum, assafœtida, clematis, agaricus, and *graphites.*

We find the following symptoms recorded under the digestive system. Hot, red, and dry lips; dryness of mouth with thirst; tongue red and dry—sometimes coated yellow—tongue and gums sometimes ulcerated (for which it was given by Dioscorides); constriction, hoarseness, and rawness in throat; persistent nauseous taste; eructations, sometimes bitter, at other times tasteless; appetite sometimes increased, at others diminished; nausea and vomiting, for the cure of which it has been successfully employed by Schrœder; hæmatemesis, for which it was prescribed by Dioscorides; pressive pain and warmth at epigastrium; colic; acute abdominal pains at various parts; pain and distension in hypochondria; pain in spleen (the drug is much used by the Hindoos for enlarged spleen); heat, pressure, and pain in liver; great, general distension and flatulence; inflammation and ulceration of rectum; dragging sensation in rectum; hæmorrhoidal pains; crawling, pricking, and burning in anus, which is morbidly sensitive to the passage of the fœces; dysentery; hæmorrhoids; stricture of the rectum; stools generally very yellow; said to act as a purgative when constipation arises from insufficient flow of bile, or atonic state of intestinal walls.

For digestive system compare with *aconite, lycopodium, pulsatilla, colocynth, arsenicum, bryonia, belladonna, ipecacuanha, nux vomica, mercurius.*

The above symptoms are nearly identical with those of *aconite* and *lycopodium,* while they strongly resemble those of *pulsatilla, colocynth, phosphorus, arsenicum,* and *bryonia.* They also bear some likeness to the corresponding symptoms under *belladonna, ipecacuanha, nux vomica,* and *mercurius.* But we no longer can trace any noteworthy resemblance to the symptoms of *graphites, agaricus, conium, plumbum, platinum,* or *aurum.*

The provings of the chest symptoms, alike respiratory and circulatory, are in a very unsatisfactory state, so far as I have been able to discover. We find epistaxis mentioned, which, according to Schrœder, it will also arrest; but, considering the tendency of *aloes* to produce cerebral congestion, it may be questioned whether in the present case this ought not rather to be considered a head symptom. Then we have sneezing and coryza, a general feeling of

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oppression and anxiety, along with acceleration of the pulse; hoarseness, and occasional inclination to cough. Congestion of blood to the chest is also mentioned by some provers, together with flushes of heat over the entire body. It is obvious that with such meagre provings any comparison which we may institute in this respect between the action of *aloes* and that of any other drug must be very imperfect, but it is equally clear that so far as they go the above symptoms closely resemble those of *aconite, lycopodium, arsenicum, phosphorus,* and *ipecacuanha.*

The action of aloes on the urinary system is variable, as at times the urine has been hot and scanty, while again on other occasions the urinary secretion has been large in amount and yellow and turbid, or, again, mixed with blood. Scalding during micturition has also been noticed in some cases. All these symptoms also occur under *aconite, pulsatilla, lycopodium, arsenicum,* and *phosphorus.*

Under the head of generative system we find aphrodisiac symptoms strongly marked in both sexes, and menorrhagia in the female, as well as uterine congestion and miscarriage—very much, in fact, the symptoms of platina, which here approximates more closely to *aloes* than any other drug with which I am acquainted; but it deserves to be compared likewise with *lycopodium, phosphorus, arsenic, aconite,* and *pulsatilla.* In some respects it ought rather to be *contrasted* with the last named. *Colocynth* here presents an absolute contrast.

With regard to morbid anatomy, the chief appearances which have been noted are those of congestion of the bowels, particularly of the large intestine, and of the pelvic viscera generally, but congestion of the lungs and brain have been recorded in some cases. Clinically it has been found especially useful in cases of dysentery, piles, menorrhagia, and some forms of hepatic disease—that is to say, just in the very diseases which, according to former works on Materia Medica, contra-indicate its exhibition, and in some cases of rheumatism, headache, falling out of the hair, and deafness.

Having thus briefly summarized a few of the leading actions of *aloes,* and seen with what drugs it admits of comparison under each system, as well as taken a short view of its clinical action and post-mortem effects, we must endeavour to apply the knowledge thus gained, so far as it goes, to discovering the organopathy of the drug, and

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further ascertaining for what regions, as well as for what tissues and organs, it has a special affinity.

The post-mortem appearances, so far as recorded, point to little more than congestion of the alimentary canal, especially at the lower extremity, but to this clinical observation and provings have enabled us to add congestion of all the pelvic viscera, and occasionally of the lungs, and meninges of the brain.

I am not aware that any rash has been observed as the result of the administration of *aloes*, but that the drug exerts some influence upon the skin seems certain, since it clearly arrests the falling out of hair in many cases. We might also have been led to expect some action on the cutaneous system, by noticing its similarity to graphites in some respects.

While we started with a comparatively large number of companion drugs with which to institute comparisons, we have seen that, as we passed hastily from system to system, they have for the most part, like earthly friends in the fable, dropped off one by one at each successive stage of our progress, so that of the 17 with which we started at its commencement, 6 only have accompanied us to its close. These are named in order of resemblance, *lycopodium*, *aconite*, *arsenic*, *phosphorus*, *platina*, and *pulsatilla*.

Now, of these we may note—

1. *Lycopodium* specially acts upon mucous membranes, and also exhibits an elective affinity for the liver and chylopoietic viscera.

2. *Arsenic* acts specially on the gastro-intestinal mucous membranes. Here we have one region of the action of *aloes* clearly indicated by comparison no less than by observation. Again,

3. While *phosphorus* certainly has an action very similar to that of *arsenic* on the gastro-intestinal mucous membrane, it has also a distinct aphrodisiac action of its own.

4. *Platina* is chiefly remarkable for its action on the pelvic organs, especially those of generation in the female. Here we find the analogues of *aloes* in reference to another sphere of action, viz., emmenagogue and aphrodisiac, and also to its most characteristic seat, namely, the pelvis.

5. The action of *pulsatilla* is over the mucous membranes generally, likewise on the organs of generation; hence the great resemblance we have noticed to *aloes*, though it is right to observe that as regards the genera-

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tive system the action of *pulsatilla* is very frequently, if not most commonly, in an opposite direction to that of *aloes*. The relations between *aloes* and *aconite* appear to me to require a little more careful consideration.

The control which *aconite* exercises over the circulation must be ascribed to the direct or indirect action of the drug upon the heart. Besides this, it appears to act upon the system of involuntary muscle generally, as shown by its power in dilating the pupil, which it does, though to a less extent than *belladonna*. Now, the action of *aconite* is very much more rapid than that of *aloes*, and its cerebral action is far more marked, both of which facts lead to the conclusion that the former acts through the medium of the nervous system—an inference which derives greater probability from the well-known anodyne properties of *aconite*.

The drug we are now considering (*aloes*) possessing no anodyne properties, and exhibiting a less distinct cerebral action, while, at the same time, it seems to affect the circulation, probably does so by its direct action on the involuntary muscular tissue contained in the coats of the vessels, and as its action on the circulation is so very much less distinct than that of *aconite*, it seems worth while to inquire whether it exerts any direct influence on the heart itself at all. No doubt, the heart is an involuntary muscle, but it possesses the striated instead of the non-striated structure, which may possibly make some difference as to the drug influences to which it is amenable.

We may, I think, sum up the organopathy of *aloes* thus: The structures upon which it acts are—

1. Mucous membranes generally, and in particular those of the pelvic viscera.

2. Involuntary muscle, especially that of the pelvic and cranial blood-vessels.

The sympathy so well known to exist between the brain and its neighbouring structures and the pelvic organs of generation, may help us to understand the influence of the drug on two such distant sets of vessels. Its action in arresting falling out of the hair may admit of explanation by its influence upon the small involuntary arrectores pilorum muscles at the root of each hair.

I should describe its primary action as tonic, its secondary action, or the action of a poisonous dose, as relaxant, and to this relaxant action on the muscular walls of the

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vessels I should ascribe the pelvic and cerebral congestion which have been recorded.

To its tonic action on the same tissue I ascribe its value in medicinal doses in the treatment of piles and menorrhagia, and it is noteworthy that it is in those cases of the last-named ailment which are accompanied by determination of blood to the head that this drug has been found especially useful.

In dysentery, with well-marked inflammatory symptoms, and much flatulence, *aloes* have been found useful, and these two conditions would seem to be especially amenable to this tonic action of *aloes* on the non-striated muscular system of the coats of the vessels and intestines, while a similar action on the mucous membrane of the alimentary canal lessens the undue secretion of mucous slime passed with the discharges.

Lastly, the forms of simple diarrhœa and dyspepsia to which *aloes* have been found most appropriate, are those accompanied with flatulence and other indications of an atonic state of the gastro-intestinal walls. Its influence on the liver may, perhaps, be further explained by the sympathy known to exist between that viscus and the rectum.

Such are the observations I have been able to collect upon this drug. I cannot but regret, that, meagre and brief as they are, they should so far have trespassed upon the time due to other more valuable and more original communications.

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HOMŒOPATHY IN THE TREATMENT OF MALARIOUS FEVERS.

By Mahendra Lal Sircar.

Mr. President and Gentlemen—Honoured Colleagues,—I can scarcely thank you sufficiently for the honour you have done me in inviting me to contribute my mite in the shape of a paper on some subject relating to homœopathy at this your annual gathering for 1874. When I recall to mind the history of these congresses, which I have watched with intense interest, when I recall to mind the great men who had presided and the great men who had contributed papers at these congresses in times past, and when I realise in my mind's eye (which alone I can do at present, being prevented by a variety of reasons from accepting your president's kind invitation), when I realise in my mind's eye the meeting of the present congress—presided over by a gentleman of rare accomplishments, to whose noble translations the English-speaking world is indebted for its acquaintance with the thoughts, experiences and reasonings of our illustrious Master,—thoughts, experiences and reasonings which have removed for ever the landmarks of mystic, and laid the foundation of positive medicine; and to whose valuable "Lectures" the world is indebted for one of the soundest expositions of homœopathy extant:—When I realise in my mind's eye the other gentlemen present, some of whom, like the worthy president, have, by their writings and researches, acquired names which are heard of in every part of the civilised world, and others—the younger band of workers—nobly striving to add to the stock of knowledge and to advance the cause of the greatest truth yet discovered in the domain of medicine:—When I realise in my mind's eye all this, I am humbled in the dust to think that you did not forget to invite an obscure man in an obscure corner of the far East, and to invite

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such a man to contribute to your intellectual enjoyment. Not for my own sake, nor even for yours, honourable colleagues, but for the sake of the truth which we all worship and the system which enshrines it, do I hasten to comply with your kind request. Yes, Gentlemen, it is simply to bear witness to the truth and to show that truth when understood has charms for even the meanest intellect, that I have ventured to lay before you this humble contribution of mine, which otherwise would be ridiculous in comparison with the contributions of the other gentlemen.

I have selected for my subject "Homœopathy in the Treatment of Malarious Fevers," for the reason that we have at present in Bengal a deadly epidemic of malarious fever which has been devastating the country for some time past, and does not as yet show any signs of diminution of its virulence, and that this, in common with epidemics in general, has been instrumental in demonstrating, not indeed the utter futility of the old system, but the vast superiority of the new over the old. The limits of this paper will not allow me to dwell at any length on the causation or even on the nature of the disease. The causation is, in spite of the efforts of Government and of private individuals to discover it, still shrouded in the densest obscurity; but the nature of the disease, though mistaken by a few eccentric medical men, is patent to every one who has eyes to see and a modicum of common sense in their heads. Whatever malaria may be, whether a material poison, or a mere influence, whatever its immediate and remote causes, in all which matters there may be and has been the widest divergence of opinion, there is perfect unanimity about its effects on the human system. The fevers, the cachexia, the concomitants, and the sequelæ, which come on in its train, are all so marked and characteristic, that even laymen do not fail to recognise their peculiar features, and refer them to their true origin, though the origin of that origin itself may not be easy to trace.

The grand characteristic of malarious fevers is their periodicity—their recurrence after definite or indefinite periods of lull as it were, of apparent health, at least, of apparent apyrexia. A typical paroxysm of fever consists of three distinct stages, the cold, the hot, and the sweating, which are followed by a period of apyrexia of a few hours

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or of a day or two, which again is succeeded by another paroxysm of the same character, and so on, for a number of paroxysms constituting an attack, after which there is a longer lull of a few days or even months, to be followed by another series of paroxysms or another attack. In this way attack after attack follows, either gradually diminishing in intensity and duration, which is a rare thing, especially when the patient continues to reside in the malarious locality, or gradually increasing in severity, till the intermissions are mere remissions, and the paroxysms, though less in violence, become more obstinate and more exhausting, in other words, till the intermittent gives way to the remittent type. Sometimes the order of things is reversed, the disease at first appearing as remittent, and afterwards becoming converted into an intermittent form. This is certainly the more favourable course of the two. As a general rule, it is the degree of virulence or concentration of the malarious poison or influence which determines the type of the fever; the more virulent and concentrated the malaria, the greater being the tendency to the remittent variety of the disease. We say as a general rule only, because we have seen cases of the strictly intermittent type in which life has been compromised sometimes in the pre-febrile, sometimes in the post-febrile stage, and this in the course of one or two days only. In these cases the malaria must certainly have been of the most concentrated and virulent form, notwithstanding that the fever was of the intermittent character. These acute cases are of the gravest description. They occur when the epidemic is at its height. In their acuteness, severity, and mortality they resemble cholera.

The varieties of these types of malarious fever are endless, and it is needless in this paper to recount them. I shall just touch upon the concomitants and the sequelæ. It is difficult to say whether the primary action of malaria is upon the blood, and through the blood upon the ganglionic nervous system, or upon the ganglionic nervous system and through it upon the blood-forming organs. Whether the one or the other, the result is the same—destruction of the red corpuscles of the blood and the arrest of their genesis. I have seen healthy, robust men, with no lack of red blood in their system, blanched after a few days' residence in a malarious district, before even the symptoms of the fever had been quite developed, and long

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before either the liver or the spleen had become enlarged. Simultaneously almost symptoms indicative of derangement of the ganglionic nervous system, especially of that part presiding over the primæ viæ, make their appearance, such as loss of appetite, defective secretion, costive bowels, nausea with or without vomiting, &c. Next in order of sequence is the implication of the animal nervous system, as manifested by extreme nervous and muscular debility: this is probably through the ganglionic nerves controlling the vessels supplying the cerebro-spinal centres. The symptoms of implication of the nervous system, ganglionic and cerebro-spinal, indicate not congestion but exhaustion and defective nutrition from an improperly oxygenated and spanæmic blood. I maintain that primarily there is no congestion of any of the viscera, cephalic, thoracic, or abdominal. I maintain also that congestion of the abdominal viscera much more frequently ensues than congestion of the thoracic, which again is much more frequent than congestion of the cephalic.

It is difficult to say, however, which of the abdominal viscera it is which suffers the earliest. If we trust to physical signs alone, apart from vital symptoms, the spleen would appear to suffer first. But attention to the vital symptoms, a right interpretation of all the phenomena of these fevers, leads to a different conclusion. It would thence appear that the mucous membrane of the digestive tract suffers earliest in the order of sequence; the liver next, or simultaneously; and last of all the spleen. The spleen, in fact, appears to me to act as a safety-valve to the disturbed functions of the alimentary mucous membrane and of the liver. I have accordingly always looked upon the early enlargement of the spleen as a salutary sign. And I have almost invariably seen that in these fevers the gastric and the hepatic derangements continue obstinate, so long as there is no perceptible enlargement of the spleen. The first effects, therefore, of the enlargement of the spleen are to divert the morbid action from the stomach and the liver, and to avert the tendency to vomiting, diarrhœa, and dysentery. Again, the assumption by these fevers of the remittent type seems to me to depend upon the want of an adequate safety-valve action of the spleen. And it is a notorious fact that it is in the intermittent variety of the disease, that this organ is found early and the most frequently enlarged. All that is said

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here with reference to the salutary safety-valve action of the spleen, is of course with reference to the early stages of the disease. The organ, in the course of the disease, may become so enormously enlarged that by its very bulk it will seriously interfere with the functions of the neighbouring and even of remote organs. Thus by its pressure upon the stomach it will interfere with digestion; by its pressure upon the transverse and descending colon it will impede its normal peristalsis and give rise to alternate constipation and diarrhœa; by its pressure upon the liver, especially if the latter be enlarged, it may interfere with its secretion, and thus cause jaundice; by its pressure against the diaphragm, and through it upon the lung and the heart, it may give rise to a variety of abnormal sensations, pains and stitches, palpitations, breathlessness, &c.

Though functionally the liver suffers earlier than the spleen, it does not become enlarged, as a general rule, till after enlargement of the latter organ, and even then it is not in all cases that it becomes enlarged. The number of cases in which there is both enlargement of the spleen and of the liver is small compared to the number of cases in which there is enlargement of the spleen alone, and the number of cases in which there is enlargement of the liver alone without enlargement of the spleen is much smaller. I cannot state the exact ratio of these cases. On a rough estimate I should think that in about 50 per cent. of cases there is enlarged spleen, in about 25 per cent. there is both enlarged spleen and liver, and in 15 per cent. there is enlarged liver alone, and in about the remaining 10 per cent. there is apparently no organic complication.

Next to enlargement of the spleen and the liver we have dropsy, dropsy of the general cellular tissue, as well as of the peritoneal cavity. Anasarca is much more frequent than peritoneal dropsy; and both, as a general rule, seem to arise as a direct consequence of the enlarged liver and spleen, and of the impoverished blood. The pressure of the liver upon the vena cava descendens and of the liver and the spleen upon the mesenteric vessels, impede the adequate return of the blood from the lower extremities, and from the intestines, and the blood being poor in red globules and fibrin and more full of water, the necessary consequence of this state of things is the

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effusion of serum into the cellular tissue and into the peritoneal cavity. The condition of the blood alone has, in many cases, determined the dropsies, without any help from the mechanical pressure of the organs in question. I have had considerable experience of the present epidemic, and had under treatment several cases of dropsy of the general cellular tissue and of the serous cavities, as the sequelæ of malarious fevers, but in no instance could I detect any disease of the kidneys. And although I would not be justified in pronouncing the absence of organic renal disorder as universal in cases of malarious fevers, yet I can assert thus much that, if it is developed at all, it must be of the rarest occurrence. This, I believe, is accounted for by the fact of the entire removal of the kidneys from the influence of circulation in the portal system.

The next sequelæ that I have to notice are disorders of the alimentary canal, chiefly the small and the large intestines, resulting in diarrhœa or dysentery or both. Diarrhœa, as I have said above, may arise from mechanical pressure of the enlarged spleen and liver upon the intestines; but it may arise independently as a consequence of the malarious cachexia. There is often a peculiar form of diarrhœa in which there is invariably aggravation from oleaginous and fatty foods. In these cases that small organ, the pancreas, which does not seem to have received the degree of attention from pathologists and physiologists its importance in the economy of digestion demands, appears to me to be at fault. Its intimate relation to the portal system would lead us to expect that it must suffer in all cases of malarious fevers, and that it does suffer is evidenced by the fact that fatty food is not well borne in these fevers. Dysentery is a rare sequela of malarious fevers, and in my opinion is generally a result of heroic treatment. It may, however, like diarrhœa, arise independently, and then it is a most serious complication.

The last sequela that I shall notice is what goes by the name of cancrum oris. This consists in ulceration and sloughing of the mouth. It has its start either in the mucous membrane or skin of the lips or cheeks, whence it gradually invades the neighbouring tissues, finally attacking bone (superior or inferior maxillary), or it may have its start in bone and thence radiate within and without, involving the structures in one wide-spread gangrenous

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inflammation. This most serious and frightful complication is rarely a phenomenon of the early stages of malarious fevers, and, in fact, it is rarely a phenomenon of genuine malarious fevers, that is, of fevers left to themselves and not much mismanaged. It is, however, not an uncommon attendant of mismanaged cases, even in their early stages, especially of cases under kavirajs or native physicians who pretend to practise after the principles of old Hindu Medicine, and use preparations of mercury extensively in almost all diseases. Cancrum oris may arise in the natural course of malarious fevers from exposure and from the free indulgence in acids and acid fruits; but my persuasion is that, in the majority of instances, it arises from the abuse of mercury in these fevers. The very use of mercury (in massive doses) in these fevers is, in my opinion, its unpardonable abuse, especially when the spleen has become enlarged and the anæmia is fully developed. The susceptibility to the action of mercury in malarious fevers is fearfully increased, and sometimes a single dose of calomel given with a purgative has brought on dangerous salivation, culminating in cancrum oris.

Such is a rapid sketch of the fevers which, for the last twenty years, have been devastating village after village of the finest and richest province of India. The epidemic, by the unwonted virulence it has manifested, by its wholesale invasion of every living soul, by the unprecedented mortality that has followed in its train, by the rapidity and certainty of its progress from village to village, has stirred up the community to its innermost depths, and naturally roused its vigilance towards the capabilities and resources of the medical profession. The various systems of treatment which divide the profession are under trial in a grand experiment, and it is both interesting and instructive to note the result of this trial. This is what I intend to lay before you, but before doing so I would just state for your information what the various systems of treatment are that are struggling for existence and mastery in this country.

Taking them in the chronological order of their existence, we have first of all the indigenous system founded on the Ayurveda or the ancient Hindu medical writings. This is a rude form of allopathy, and is characterised by the wildest polypharmacy. The practitioners of the system are invariably Hindus, and are called Kavirajs or Vaid-

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yas. We have next the Arabian system, founded upon the writings of Avicenna and other old Arabian authorities. This also is a rude form of allopathy, and characterised, equally with the former, by polypharmacy, with this difference, that it deals more with food medicines, and less with virulent poisons. The practitioners of this system are called Hakims, and are both Mahometans and Hindus, but chiefly the former. We have thirdly the European allopathic system, which has come into vogue since the conquest of India by the British, but which has obtained a secure footing since the establishment of the Calcutta Medical College. Of this system I need not speak anything to you, as you are too familiar with it. We have last of all the system of Hahnemann, the exact date of introduction of which into India I have not been able to trace, but which, if I may say so without presumption, has begun to command respect, if it may not be said to have yet obtained a secure footing, since the conversion of your humble colleague. Homœopathy has not yet gained many professional votaries in this land. In fact the number is so small that it may be counted on one finger. But it can count non-professional votaries by hundreds and even thousands. And while the interest in the system is being mainly kept up by a few professional devotees, and by the Journal published in Calcutta, devoted to its cause, it is to some extent also sustained by non-professional practitioners, who deserve all praise for this good service in the absence or dearth of professional men. I may here mention, in a general way, that the Hindu system is in favour with the orthodox Hindus, the Arabian system with the Mahometans, the European system with the Europeans and the educated classes of the Hindu and Mahometan communities. The Hahnemannian system is just making its way, and is in favour with a few Europeans, a pretty large number of Hindus orthodox and heterodox, and a few Mahometans. It is not a little curious that the Europeans are the most conservative in the matter of treatment, the Hindus the least so. I should think the Hindus have been most unjustly blamed for their stereotyped character. They are, in fact, of all the nations in the world, the most susceptible of reform and progress, if only they are convinced of the genuineness of the reform and the reality of the progress; and they are very open to conviction.

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At the time the epidemic first broke out, the European allopathic system, by the fact of its being built upon a number of the most positive and progressive sciences, and even by the progressive character of its therapeutics, kept up by the spirit of research, had already asserted its immense superiority over the Hindu and Arabian systems. In no other disease was the utter helplessness of these systems proved more than in periodic fevers, in the treatment of which the European allopathic system was looked upon as wielding the wand of the magician. When the epidemic broke out with fevers characterised by periodicity, the eyes of the sufferers and of every body were naturally turned towards this system, and its professors and practitioners fearlessly faced the enemy, in full reliance upon their infallible specific, and the result for a time was truly magical, but only for a time. Soon the system was startled out of its dream of infallibility. So long as quinine succeeded in suppressing the fevers, the system was in triumph. But the human organism became accustomed, and therefore blunt to the action of the antiperiodic, and the fevers made their wonted appearance, less and less to be influenced by it, till a stage arrived when the exhibition of quinine or bark in any shape became a positive evil, causing aggravation of the fever, and bringing on other unpleasant symptoms in its train.

Quinine failing, the system became impotent, and the people could not fail to notice the impotency. The battle, of course, was not given up, as indeed it could not be given up with any decency. A host of drugs was summoned to aid the specific, which of course was never omitted from any prescription, and the dose of which entirely depended upon the fancy or whim of the practitioner. Some would prescribe it in what they are pleased to call small, tonic doses, and some in heroic, sedative, antiperiodic doses. In the one case the dose was a grain or two, or even fractions of a grain; in the other it ranged from 10 to 90 grains. Among the auxiliaries the mineral acids, iron, strychnine, arsenic, iodide and bromide of potassium, played the chief part. Some practitioners would abstain from purgatives. The majority, however, could not do without them. The action of the bowels must be kept up, so as to derive, as they said, from the liver and the spleen. And when diarrhœa was thus produced, astringents were exhausted, with or

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without opium. When from this procedure the bowels became constipated, purgatives were again had recourse to; and thus the unfortunate sufferers oscillated from artificial purgation to artificial constipation, till either diarrhœa or dysentery of an inveterate character was established to terminate their miserable existence.

Very few people could escape with their lives, under the above treatment, to have recourse to others, and fewer still could really recover. Grave sequelæ mentioned before would gradually but inevitably make their appearance, each of them threatening to take away the lives of the patients with the certainty of fate. Under these circumstances, it is really lamentable to behold the way in which orthodox physicians conduct themselves in their struggle against the disease. The same medicines are given in all possible combinations, and with all possible variations of dose. In addition, we have now external applications over the regions of the enlarged or congested viscera,—counter-irritants in the shape of blisters, the tincture or the ointment of iodine, &c. Some very stupid practitioners do not hesitate to have recourse even to leeches to subdue the so-called inflammations or congestions of the internal organs, which they ignorantly believe are the cause of all the mischief. I have seen cases in which blisters became sloughing sores, and, extending down to the ribs, have been the cause of death of the most frightful character. I have seen cases in which the bleeding from leeching has produced hopeless prostration, or aggravated hundredfold the already existing cachexia.

Such has been the result of the European allopathic system in the present epidemic, a result which has been summed up by a Government Commission, "appointed to enquire into the causes of the Epidemic, its course, and the best means of checking its further progress," in the following words:—"It must be borne in mind that do what we may, the disease must in all probability run a certain course, *which is neither to be accelerated or retarded by any means within our reach.*" As for the other allopathic systems, Hindu and Mahometan, we have already said that without Peruvian bark and its alkaloid they are utterly helpless, and if the Kavirajs do succeed, it is because of their surreptitious use of these drugs in strange combination with their own strange compounds.

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So that we can leave them out of consideration altogether in the present comparison. When, therefore, we speak of orthodoxy, or the orthodox system or school, we mean the European allopathic.

What has been the result of the homœopathic system of therapeutics in the treatment of the present epidemic? At the time the impotency of the orthodox system was discovered by the people, we had, properly speaking, no regular professional practitioner of homœopathy. But we had a layman of remarkable intelligence, of untiring energies, and of a philanthropic disposition, who had become a convert to the new doctrine, and who endeavoured by the aid of books to master its principles, and apply them in the treatment of cases which had become incurable under the old system, or pronounced incurable by its practitioners. As if under the guidance of Providence, the first patient that presented himself to him was one long suffering from malarious fever and spleen. Babu Rajender Dutt, with no experience of the new system in the dreadful cases of fever of the present epidemic, could think of no other drug to give to the patient in question than quinine. But as the patient saw that he was going to have quinine, said that he had consumed several phials, and would not have any more of it. He would be thankful if the Babu had anything else to give him. This induced him to study the case with some minuteness, so as to individualise it to the best of his ability, and he prescribed some homœopathic medicine; it was *arsenicum* in the present case; and what was the result? The man returned in three or four days to report that there was no return of the fever, which had become his daily troublesome companion for a long long time. The remedy was repeated, I cannot say for how many days, but the cure was complete. The fever, and with it the spleen and other attendant evils, vanished like a charm. This was the first case of remarkable success, and it brought in many more. The Babu's lodging at Chandernagore, where the first victory was gained, and afterwards his dwelling house at Calcutta, came to be thronged with patients of all descriptions, but chiefly those suffering from malarious fevers, patients who had derived no benefit from the old system. The majority of these patients recovered, and the fame of the Babu and of homœopathy spread far and wide. And it was then, but

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not till then, that my attention was arrested by the unparalleled success thus achieved, though I must observe it to my shame, that Babu Rajender Dutt is almost my next door neighbour; such is professional jealousy and professional pride! However, once convinced of the efficacy of the despised infinitesimals, I studied the system in right earnest, and finding it of incalculable superiority over what I was taught to believe as the only rational system of medicine, I proclaimed my convictions at a meeting of the Bengal Medical Association in February, 1867, disdaining to ignore the life-giving truth by stifling my conscience, with what result is probably not unknown to you.

Since the year 1867 I have had a most extensive experience of homœopathy in its relation to the present epidemic, having had under my immediate observation upwards of 5,000 cases, and I can most confidently testify to its immeasurable superiority over the orthodox system. Not to speak of the complicated cases with the grave sequelæ mentioned before, in which orthodoxy, with its heroic armamentaria, is not only helpless but mischievous, in the management even of the ordinary cases homœopathy has decided advantages, inasmuch as it can do without purgatives, blisters, leeches, and unnecessary shaving of the head—no small relief to patients in general and to children and female patients in particular. With a few precious drugs homœopathy exercises a marvellous control over the morbid actions wherever manifested.

Before proceeding to sketch the treatment of these fevers let us first put clearly the problem before us. From the description we have given above based upon a large observation of treated and untreated cases, it must have been evident that these fevers are of variable durations—each case consisting of a series of attacks of variable durations, following each other at variable intervals from a day or two to a week or a month or even a year;—each attack again consisting of a series of paroxysms of variable durations, and following each other at variable intervals from a few hours to a day or two. Such being the nature of the fever, is it possible to arrest it at the very first attack, and the attack at the very first paroxysm? So far as my experience goes I do not think this is possible, so long as the patient continues to reside in the affected locality; and even his removal to

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a healthy locality does very seldom let him off without a series of attacks, which sometimes may be as grave as if he were in the affected locality itself. Even assuming that removal could secure exemption from future attacks, how can we advise the exile of a whole people? Accepting then the hard fact that people, as a general rule, must reside in their homes, I must state it as an equally hard fact that so long as this is the case, I have not seen a single case of malarious fever which has terminated in one attack, far less in one paroxysm, under any system of treatment. I have thought it advisable to make this statement *in limine*, because I have been annoyed to find that some homœopathic practitioners seem to labour under the delusion that if a right selection is made according to the totality of symptoms then the disease could be extinguished in the bud. If this delusion were confined to themselves it would be harmless; but these practitioners make people share in it, and this gives rise to unnecessary disappointment, which in its turn brings unmerited disgrace upon the system.

In the treatment of these fevers, it is important to bear in mind the stage of the disease at which the treatment is commenced, and the variety of the disease. Roughly speaking the whole duration of a case of malarious fever may be divided into two stages, the acute or the early stage when the sequelæ are not yet developed, and the chronic or the later stage when these become developed. The character constituting the variety of the fever is either intermittent or remittent. The determination of the stage and the character is easily made, but still it is necessary to bear them in mind. For though we have to rely upon symptoms in their treatment, yet the same symptoms have not the signification and bearing and consequently the same importance in the different stages and varieties. I have seen *aconite* prescribed not only in the last stage, but in the last state of the last stage, simply because the patient was in a slight paroxysm of fever! After the stage of the disease itself we have next to consider the stages of a paroxysm in reference to the order of their succession, and the order of their appearance according to the hour of the day.

In the early stage, during a paroxysm, especially of the remittent variety, *aconite* is a remedy of the first importance, in subduing heat and allaying thirst. When

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the disease becomes established as an intermittent the sphere of its usefulness becomes narrowed into those cases only in which the accessions come on in the evening.

When along with high fever there are symptoms of cerebral congestion indicated by headache or heaviness of the head with blood-shot eyes, then *belladonna* after *aconite*, or in alternation with it, is of service. From the remarkable success that attends the employment of *aconite* and *belladonna* in the rapid subdual of the febrile paroxysm without the time-honoured refrigerants, diaphoretics, and purgatives, allopaths have come to recognise the value of these drugs, and use them in the same conditions, but in doses, however, too minute to conceal the source whence they are borrowed.

When there is congestion of the liver, indicated by tenderness in the hepatic region, with or without jaundice, *bryonia* after or in alternation with *aconite* is remarkably useful in relieving the congested organ. *Bryonia* is also useful in congestion or inflammation of the bronchial mucous membrane and even in pneumonia, but if it does not succeed well in the latter, *antim. tart.* seldom fails; and in case of hepatization *phosphorus*.

Bryonia of all drugs is of the greatest service in the remittent variety of the disease. *Bryonia* and *rhus toxicodendron* in alternation are the remedies I have chiefly relied upon in those severe remittents which, in every thing excepting the peculiar eruption, resemble the typhus of Europe. In remittents similarly resembling the typhoid or enteric fever, that is, when there is diarrhœa, whether with or without ulceration of the small intestines, I have derived the greatest benefit from *baptisia*. When with the looseness there is tympanitic distension of the abdomen *china* gives me good service; failing with this I use *antimonium tartaricum* with success.

Cases attended with obstinate bilious vomiting are benefited by *eupatorium perf.* which seems to have a remarkable control over these cases, especially when the accessions come on in the morning, with thirst commencing long before the chills, and continuing throughout the chill and the heat, or when they are of the double tertian type, one day occurring in the morning and another day in the afternoon, the morning and afternoon paroxysms being respectively severer and lighter. But even apart from this type I have found *eupatorium* useful in cases where

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the irritability of the stomach is so great that not only nothing is retained, but there is vomiting after even the slightest draught of water, so that the very sight of water is dreaded by the patients while suffering from burning thirst. One such case has been recorded in the *Calcutta Journal of Medicine*, and this case gained me a medical man for a convert.

I need hardly tell you what *ipecacuanha* has done in the way of giving converts to homœopathy. It is very good indeed where there is persistent *nausea*, but it is hardly useful where there is obstinate *vomiting*. In these cases *eupatorium*, as just said, *antimonium crud.*, *antim. tart.*, *nux. vom. arsenicum*, &c., are more useful, of course being selected according to other indications as well.

In the very acute cases when the patient complains of so much heat that he seems to think fire to be issuing from his mouth, his nostrils, his eyes, and his ears, and when along with this there are burning thirst, heat of head unrelieved by cold applications, great irritability of temper and restlessness, *chamomilla* has acted like a charm. *Cham.* has been also remarkably useful in cases where there were nausea, bilious vomiting, bilious diarrhœa, with colicky pains in the abdomen; also in fevers which had become aggravated by fits of anger or chagrin.

In acute cases with great heat and thirst, burning of the skin, great restlessness, with a despondent mood, *arsenicum* in lower potencies has often been useful. *Arsenicum* is particularly useful in cases where along with the symptom already mentioned, there is diarrhœa, which we have reason to suspect has been brought about by indulgence in fruits and cooling things in general.

I have found *antim. crud.* useful either when during the paroxysm there is considerable gastric derangement, such as loss of appetite with loathing of food; constant, loud, bitter eructations; nausea with vomiting of mucus and bile, with or without diarrhœa, and the tongue coated white; or when the fever sets in at noon, with or without chills, and is accompanied during the heat with somnolence.

For fevers returning at the same hour, but chiefly just after noon, and characterised by vomiting and diarrhœa, but chiefly vomiting, of bile or of the ingesta, and colicky pains in the small intestines, with or without, better with, canine hunger, I have found *cina* to be a capital remedy.

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For fevers returning exactly at the same time *sabadilla* has been sometimes useful, not at all to the extent it has received credit for.

For fevers returning in the afternoon and accompanied by frequent micturition, I have found *cedron* almost a certain remedy.

For fevers quotidian, tertian, or quartan, coming on in the afternoon or evening and accompanied by loss of appetite, nausea with or without vomiting, and constipation, *nux vomica* has been particularly useful.

For fevers quotidian or tertian, and coming on in the evening or even afternoon, and attended with diarrhœa, *pulsatilla* has seldom failed us.

For fevers accompanied by stupor and obstinate constipation, *opium* (in drop doses of the mother tincture) acts almost like magic. For fever accompanied by somnolence and diarrhœa *antim. tart.* is useful. Much of the success of Grave's Mixture in fevers with cerebral congestion depends no doubt upon the homœopathicity of these drugs to the cases.

We seldom get fevers with the characteristics mentioned by Hahnemann, namely, thirst *after* the hot and *during* the sweating stage, rush of blood to the head, with distension of the veins; but when we do get them *china* in dilutions is useful.

Such are a few remedies which I have found eminently serviceable in the early stage of the disease. There are others which I might mention but whose indications cannot be given in the same laconic way. They require to be differentiated with greater minuteness and care. It is not a little remarkable that cases of malarious fevers, which come under homœopathic treatment from the beginning and which persevere under that method of treatment, very seldom in their later stages become complicated with the dreadful sequelæ mentioned in the beginning of this paper. But it is not always that we get cases to treat from the beginning. In fact, we very frequently do not get cases, till after they have been seriously mismanaged by the other systems, and unless they are too far advanced, it is in these cases that the immense superiority of homœopathy is displayed. I shall succinctly notice the remedies that have upheld the honour of our system.

Nux vomica and *arsenicum* occupy the very foremost place in this list of remedial agents. Both are useful in

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enlargement of the spleen and of the liver. The fevers of *nux vomica* recur, as already said, in the afternoon or evening, but chiefly in the afternoon. The fevers of *arsenicum* are of an irregular character generally, or they occur about or after midnight. In the fevers with predominant chills or shivering, *nux vom.* is useful; in those with predominant heat *arsenicum*. *Nux vomica* is appropriate when there is constipation or a tendency thereto; *arsenicum* when there is diarrhœa. Dropsy is no contra-indication for *nux vom.;* but *arsenicum* is *the* drug for this condition, whether it exists as anasarca or peritoneal effusion. It is not always, however, that *arsenicum* succeeds in dropsies, and it is then that we pass to the province of *helleborus niger*. Failing with these I think of *digitalis* and *ferrum muriaticum*, both in drop doses of the mother tincture. *Cantharis* has done me excellent service in restoring the renal secretion, when everything else has disappointed me.

Bryonia is generally looked upon as a remedy suited to the acute stage of malarious fevers. But I have found it of remarkable utility in cases of enlarged liver with or without jaundice, and with or without enlarged spleen. The homœopathicity of *bryonia* is all the greater when along with enlargement of the liver there is congestion of the bronchial mucous membrane. *Bryonia* is useful also in the constipation, very often present in these cases, due to deficiency of the hepatic secretion.

Similarly the use of *belladonna* should not be confined to acute cases alone. It is an excellent remedy in the advanced stages with visceral enlargements, especially when the fever is of the double quotidian type. I have found it useful in cases, when along with itching of the nostrils, there is grinding of the teeth during sleep.

In the cases of inveterate drunkards suffering from malarious fevers, and in whom along with enlargement there is cirrhosis of the liver, with jaundice loathing of food, constipation, anasarca and even peritoneal dropsy, I have succeeded in charming away this array of terrible complications with *lachesis*.

Calcarea carbonica is a remedy which has given me good service in cases, especially of children and adolescents, with enlarged liver and spleen, with a tumid abdomen dependent upon implication of the mesenteric glands, with or without anasarca, with or without diarrhœa, but especially with diarrhœa, and with fevers recurring in

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the forenoon. Young children, with these complaints and in whom there has been slowness in the development of the teeth, and of the osseous system generally, are often singularly benefited by this drug.

In the above sketch of the fever and its complications I have omitted to mention one most distressing complication, which may be present alone, or in company with other complications. This is neuralgia. I have recently had to treat a case in the person of a medical friend, in whom the neuralgia first showed itself in the brachial plexus and then in the intercostal nerves. For the first manifestation I derived, or rather the patient derived, the most unexpected benefit from *lycopodium* and for the second from *causticum*; and in fact I have seldom failed to relieve such neuralgias with the self-same remedies, other symptoms corresponding. This case is recorded in the *Calcutta Journal of Medicine* for Nov. and Dec. 1873.

Of the use of *pulsatilla* in the chronic cases I need not say much, as it may be used with the indications given above. It is useful in cases with enlarged spleen and liver. I shall only mention that it has been of remarkable benefit in cases where the paroxysm commences with toothache which continues throughout its whole duration. It is useful also in cases of enlarged Pancreas which may be detected by palpation, as well as by the peculiar diarrhœa associated with it.

Sometimes from the very beginning, but chiefly when they become chronic, malarious fevers, in this country in particular, seem to pay particular homage to our satellite, regularly appearing with every new and full moon. In these cases *silicea* is the remedy upon which I have chiefly relied, but I have also used with benefit, *calcarea carb., clematis, rhus tox., nux vom., arsenicum, causticum*, &c., selecting them according to their indications.

Natrum muriaticum has often cured inveterate intermittents, especially after abuse of Quinine, but it requires to be selected with care. One of its characteristics is inclination to sleep during the chills.

Sulphur is a remedy which we are recommended to use when other remedies do not seem to produce their desired effects. This I look upon as a superstition. Unless we can use *sulphur* according to its own indications, it will be of no earthly use whatever. One of its indications, that we have found to lead to happy results, is heat with partial sweat.

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Carbo veg. is an excellent remedy when the malarious cachexia, indicated by anæmia and prostration, is much developed, and when there has been much abuse of quinine and iron in the early treatment of the disease. It is particularly appropriate when there is diarrhœa with flatulent distension of the stomach and of the small intestines, and when the paroxysms of the fever are ushered in by extreme coldness of the feet. *Carbo veg.* is especially suitable after *arsenicum.* When the prostration is very great, an alternation of these remedies is often of greater efficacy than the use of either alone.

Though dysentery is not a frequent complication of malarious fevers when left to themselves, yet we very frequently have to treat cases with this complication as the result of heroic treatment. In such cases my chief reliance is upon *ipecacuanha* and *nux vom.* In advanced cases when there is sloughing and ulceration of the colon, we have recourse to *arsenicum, china, silicea, colocynthis, lachesis,* &c., the indications of which it will be too long here separately to point out. When the dysenteric symptoms are acute, we need not hesitate to use *mercurius corrosivus.* When we have reason to suspect that the dysentery is dependent upon erysipelatous inflammation of the colon, *belladonna* and *rhus tox.* often give excellent results.

For Cancrum oris the best remedy is *arsenicum.* After it, *carbo veg., silicea, lachesis, aurum met., aurum mur., ferrum mur.* (in massive doses), *carbolic acid* (in dilutions), *china, quinine* (in massive doses), *sulphur, hepar sulphuris,* have been found useful very nearly in the order in which they are mentioned. The best application is clarified butter, or calendula lotion or cerate. I do not think *carbolic acid* as an external application is of much use. The patients get so much disgusted with the smell that they soon lose all appetite. For the hæmorrhages that from time to time take place, strong lotions of Arnica are often useful. Failing with this I use *hamamelis.*

As yet I have said almost nothing about the dose and the dilutions I use. As the question of dilution and dose is still unfortunately one of the unsettled points in our school, it behoves me just to say with what dilutions and doses have I been most successful in these fevers. My highest dilution has never exceeded the 30th decimal, and I have seldom had to descend to lower than the 3rd. In exceptional cases I had to go to the mother tincture as

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already pointed out. My medium dilution is the 6th decimal. But I must say that in the case of some drugs, such as *carbo veg.*, *calcarea carb.*, &c., I derive the greatest benefit from the 30th. In the case of adults I use the tincture in quantities not exceeding a drop; in the case of infants and very young children, the globule, seldom more than two or three at a time.

Gentlemen, I have been admonished by our president to be brief, and I must therefore here bring to a close this paper full of short-comings and imperfections as it is. I have not enumerated, as indeed it is scarcely to be expected that I could enumerate, all the remedies that even in my own practice I have found useful in the treatment of these fevers with their formidable sequelæ which have been raging in this country with epidemic virulence for nearly a quarter of a century. In presenting this paper to you my object has been not to write a treatise, but simply to give a brief comparative sketch of my experiences with both those systems of treatment which I have practised in the fevers in question, in order to show the vast difference there is between them in point of resources and power of control over the disease; and if I have shown that the difference is all in favour of homœopathy my object has been gained.

Gentlemen, I have no doubt you must have deemed it rather strange that I should have said nothing regarding *the* drug which gave birth to our system. The fact is, I have reserved my say about it for the last. The position of Peruvian Bark and its alkaloid Quinine in the treatment of malarious fevers is the same as that of Camphor in the treatment of Cholera. If we endeavour to sum up the opinions expressed regarding the one drug and the other, we shall find them about equally divided into affirmation and negation. For instance just as according to some Camphor is the one and sole remedy for cholera in all its stages, so Cinchona is deemed to be the remedy for malarious fevers in all its stages. Again just as according to others Camphor is hardly of any use in any stage of cholera, *arsenic, veratrum,* &c., being the true remedies according to indications, so Cinchona is deemed of quite minor importance, the true remedy being found by the totality of symptoms. Between these extreme opinions, there is of course a variety of all shades, and for a beginner it is quite a puzzle to find out the true one. But the true opinion must be found out, the decision must be arrived

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at on the subject, or human lives must be sacrificed. The question is not simply of less or more prolonged suffering, but even of life or death. Medical men who have never practised in epidemic striken localities can form no idea of the gravity of the cases we have to deal with. Some of the cases, as I have said before, resemble cholera in the suddenness of their attacks and in the dreadful collapse they produce sometimes in the pre-febrile and sometimes in the post-febrile stage, and the object of prime importance in these cases is to prevent the accession. I know of no drug which can effect this with so much certainty as peruvian bark, or to speak more properly, its alkaloid quinine. I am aware of the objections which have been urged against its true febri-genic properties by Langheinz of Darmstadt and others, but whether it can or cannot produce typical intermittents I need not waste time to discuss. There is ample evidence, both allopathic and homœopathic, to show that of all drugs, this is pre-eminently one that can stamp the character of periodicity upon most of our morbid actions, and therefore this drug is *par excellence* an antiperiodic, and it is this invaluable property that is absolutely wanted in the treatment of the very grave cases we have to deal with here, as indeed, I believe, in all malarious-epidemic-stricken localities.

As the result of no less than eight years' hard experience with both the old and the new schools of medicine in the treatment of one of the direst epidemics that could fall to the lot a medical man to deal with, I have found that in the early stages we can seldom dispense with the use of Quinine. My rule is this: When the cases are of the gravest character described above, I at once exhibit the drug irrespectively of the stage of the paroxysm, in large doses during the paroxysm, and in small and repeated doses during the apyrexia. It is by this and this practice alone that we can avert death in the majority of cases. When the cases are of the ordinary character, I commence the treatment with the remedies described before, with which we undoubtedly succeed in managing the paroxysm infinitely better than our brethren of the old school. But if, in spite of this, attack follows attack enfeebling the patient, then I use Quinine as an antiperiodic, and I combine with it the tincture and the decoction of

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the crude bark, that is, the alcoholic and the aqueous extracts, to concentrate as much as possible all the virtues of the drug. The result generally is, either the total disappearance of the fever at once, or the gradual diminution of the severity of the attacks till they cease to appear. But as I have said this disappearance is seldom permanent. The fever returns after some time, with the same, or new symptoms. Quinine again may be used with success, but with each attack after its exhibition, its utility diminishes, till finally it ceases to be a remedy, and its blind, persistent use is sure to result, as it does result, in mischief. And now is the time for the full development of the efficacy of the remedies mentioned in the preceding sketch, which seem to act better after the system has been saturated with, or rather acted upon by, Quinine. The fact is, practitioners flushed with their unexpected success in chronic cases with infinitesimals alone, and absolutely without Quinine were deluded into the belief that they could dispense with Quinine altogether, at least, with its massive doses, but when the hour of *their* trial came, when people began to confide them with cases from the beginning, they began to be disappointed, though unfortunately they could not see their mistake. In spite of greater diligence in the search after the appropriate remedy, in spite of renewed endeavours to hunt after symptoms of the patient and symptoms in the *Materia Medica*, the true remedy seemed always to elude the search and mock the struggle, till the cases are made over to the allopaths who with a few doses of Quinine effect the cure. Oh! the cure is for the time being, it is said with the strained triumph of a discomfited man. However for the time being, the cure might have been, it was a cure, and that was a great thing for the patient.

Gentlemen, I verily tell you that it is bark and its alkaloid which have kept up the vitality of the Old School, and it is our disloyalty to them which has stood seriously in the way of the progress of our own School, and which not unfrequently brings unmerited ridicule and abuse upon our doctrines. In rigidly adhering to the minutiæ of our system, I am afraid, a large number of our brethren have become too exclusive and intolerant. But in our anxiety to be homœopaths we must not forget to be physicians, in our zeal to worship Hahnemann we must not cease to worship Truth wherever found.

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